THE ASTROPHYSICAL JOURNAL

THE ASTROPHYSICAL JOURNAL

An International Review of Spectroscopy and Astronomical Physics

EDITORS

GEORGE E. HALE AND EDWIN B. FROST
of the Yerkes Observatory

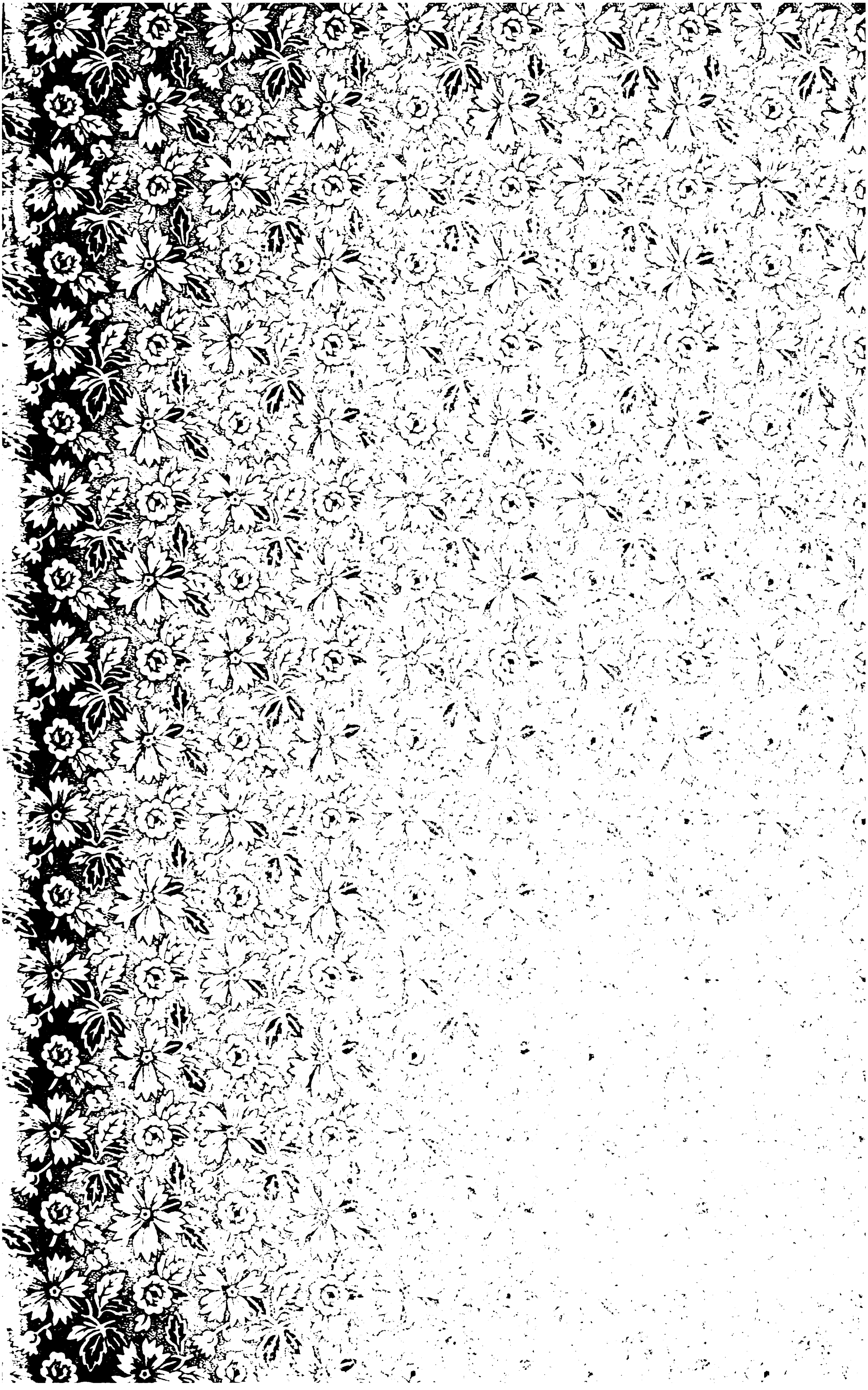
VOLUME XX

JULY—DECEMBER, 1904

CHICAGO

The University of Chicago Press

1904



THE ASTROPHYSICAL JOURNAL

THE
ASTROPHYSICAL JOURNAL
AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME XX JULY 1904 NUMBER 1

THE LOWELL SPECTROGRAPH.

By V. M. SLIPHER.

IN order to take advantage of the superior atmospheric conditions for spectroscopic researches at this observatory, Mr. Lowell, the director, ordered of John A. Brashear, in December 1900, a complete spectroscopic equipment as efficient as could be constructed. It was to be used in connection with the large Clark refractor and, pursuant to the policy of this observatory, to be devoted primarily to planetary investigations. The work to be undertaken lay along two lines: (1) to determine the rotation periods of the planets, and in particular that of *Venus*, by measurements of the Doppler displacements of the spectral lines; and (2) to study the atmospheres of the planets by comparison of their spectra with the solar spectrum. The first of these problems demanded a powerful spectrograph, as such work could be done only photographically; and the second required a universal spectroscope with which observations could be made both visually and photographically. And to deal advantageously with the light from objects as bright as *Venus* and as faint as *Neptune* and the brighter satellites of the solar system, it was necessary that the equipment should be capable of yielding spectra varying considerably in dispersion. These conditions are well satisfied by a powerful three-prism spectrograph, supplemented by apparatus carrying either of two single 60° prisms differing widely in dispersive

power, or a Rowland plane grating, and supplied with cameras and telescopes of different focal lengths. Plate II shows the spectrograph mounted on its laboratory stand and surrounded by the other parts of the equipment.

The order to Mr. Brashear was for the best spectrograph he could make. In consequence, he consulted those using his spectrographs in order to obtain any suggestions which might assist in designing and building an instrument, the efficiency of which would be the highest attainable. In design and mechanical construction it is similar to the Mills spectrograph of the Lick Observatory. It differs however, in having a collimator truss of three rods instead of four, a Huggins guiding device, and a prism-train equipped with a minimum deviation device. In planning and building the instrument, care was taken to insure great rigidity throughout, and particularly in the prism-box, and the construction is sufficiently massive to insure against flexure.

The large refractor for which the spectrograph was designed and with which it is used has an objective, by Clarks, with a clear aperture of 61 cm and a focal length of 983 cm for the photographic rays. This ratio of aperture to focus—1 to 16—had to be satisfied by the collimator lens in order that it might transmit all the cone of rays from the large objective. The length of the collimator depended upon what seemed to be the most feasible aperture and dimensions to give the spectroscope. The amount of weight that might be added to the telescope tube without danger of flexure, and the additional length of tube that the dome could well accommodate, together with a consideration of the efficiency of the instrument for planetary work, led to the adoption of a collimator of about 490 mm focal length. This gave the lens an aperture of 30.5 mm, and defined the size of the prisms and the dimensions of the spectrograph.

The spectrograph was mounted in the autumn of 1901. Complete, with the necessary counterbalances at the upper end of the telescope, it added 450 pounds to the weight of the telescope tube, which required a corresponding increase in the counterbalances on the declination axis. The mounting of the refractor is massive, and this considerable increase in the weight upon the bearings has not influenced the running of the driving-clock. To counterbalance the

spectroscope at the objective end, the rack and weights accompanying the spectroscope were mounted, but were soon removed and the tube wrapped with heavy sheet lead held in place by two ring clamps. This made the weighting symmetrical, and the telescope is in equilibrium in all positions. This counterbalance is permanent, and when the spectroscope is removed it is necessary to add an equal weight of counterbalances to the racks at the eye-end.

The attachment of the spectroscope to the telescope is as follows: A large conical-shaped cast-iron jacket, with a flange, fits over and, near its circumference, against the heavy end-casting of the telescope tube. Three large capstan-headed screws fasten the jacket to the end-casting. They are each accompanied by a pair of butting screws which, together with three on the flange for lateral adjustment, serve to set the axis of the spectroscope into coincidence with the optical axis of the refractor. The jacket tapers rapidly from 27 inches at the flange down to 13 inches at its lower end, where it supports a second casting, a cylinder which has at its upper end a flange through which six capstan-headed screws pass and rigidly join it to the jacket by a bayonet joint. The lower end of this cylinder is surrounded by a collar upon which the spectroscope proper is supported. Three hinges, mounted on the upper end of the collimator section which fits against the cylinder, swing in over the collar and clamp by milled-head screws. This gives a convenient and yet rigid attachment of the spectroscope to the refractor.

The jacket is intended to be left on the telescope. The cylinder is readily removable, but, as it does not in any way interfere with the tail-piece of the telescope when equipped for micrometric work, it is left in place on the jacket. In changing from visual to spectroscopic work, the section of the tail-piece carrying the micrometer and the focusing rack and pinion is removed, and the tube of the correcting lens is inserted in its stead. The spectroscope is then attached, and the electrical connections of the temperature control and comparison apparatus are made. The convenient arrangement of the different attachments makes the change from one kind of work to the other require only a few minutes' time.

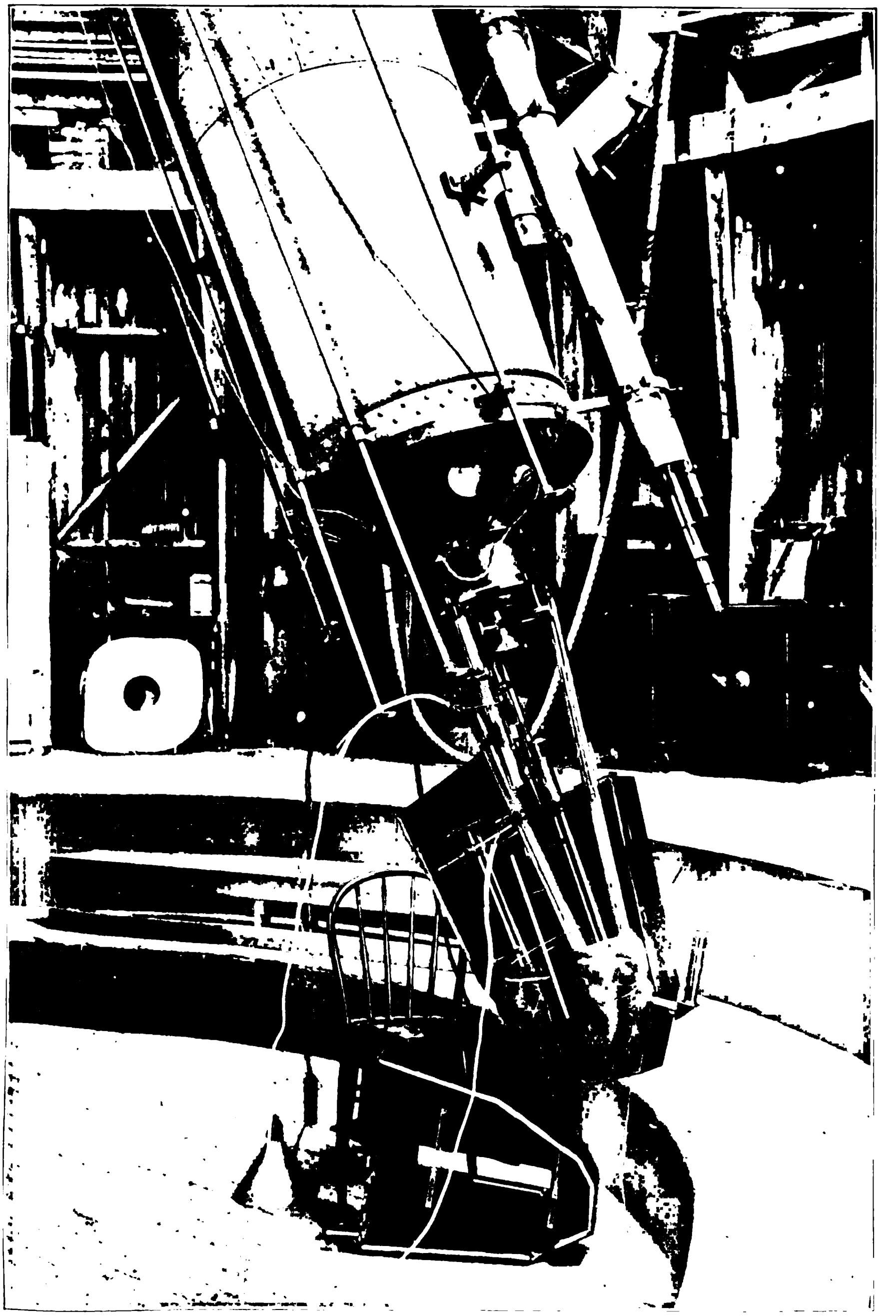
The lower end of the cylinder of the adapter carries a position circle graduated to degrees, and the end-casting of the spectroscope

has an index fastened to it so that by loosening the milled-head screws of the clamps the entire spectroscope may be rotated through any desired angle about the optical axis of the refractor. This feature of the mounting has been of great convenience in work on the rotation of planets. It permits the spectroscope to be turned so that the camera is either above or below the collimator—a change which is frequently desirable for ease in guiding.

In a mechanical sense, the principal part of the spectrograph is the collimator section. It consists of three hollow steel rods, 1¼ inches in diameter, two end castings, and an intermediate web, all united into a rigid tripod-shaped truss, which is 31 inches long and weighs 51 pounds. The ring casting at its upper end, 12 inches in diameter, carries the clamps which attach the spectrograph to the adapter. The lower end of the truss, 6¼ inches in diameter, supports the prism-box. A sleeve firmly held by the web and the lower casting incloses the collimator tube. The collimator and slit-plate may be moved as a whole in this sleeve through 70 mm by a rack and pinion, to bring the slit into the focus of any desired ray from the objective. A millimeter scale and clamp are used to set the slit in any desired position. The upper end of the collimator tube projects beyond the sleeve and is supplied with a focusing rack, a clamp, and a millimeter scale for fixing the slit in the focus of the collimator lens. An arm, 15 inches long and 1¼ inches in diameter, which is attached to the upper casting, carries the guiding tube and the sparking apparatus for furnishing the comparison spectrum.

The slit of this instrument is quite similar to that of the Mills spectrograph, save that it is arranged for guiding by the Huggins method. The jaws are of speculum metal and are highly polished. Both are movable. A screw drives a wedge which opens the jaws symmetrically, and springs close them as a backward turn of the screw withdraws the wedge. The screw has a pitch of 0.25 mm and a milled head divided into ten equal parts; thus one division corresponds to a slit-width of 0.025 mm. A wedge-shaped metal tongue is mounted immediately in front of the central part of the slit. It is supported by an arm sliding in ways at the side of the jaws, and is supplied with a block and screw to limit its motion. In this way the portion of the slit occupied by the stellar source may

PLATE I.



THE LOWELL SPECTROGRAPH ATTACHED TO THE TWENTY-FOUR INCH REFRACTOR.

be conveniently covered while the terrestrial or comparison spectrum is photographing. Just beneath the slit is a pair of jaws for controlling the width of the spectra. By turning a milled screw, accompanied by a scale, these jaws are opened and closed symmetrically.

The prism-box is semicircular in shape, 13 inches in diameter, and 2⅝ inches deep. The heavy brass base-plate is cast with a perpendicular section along its straight edge, braced beneath by two ribs. To this section is firmly united a thick circular disk worked to fit the end-plate of the collimator truss. Through the disk pass four capstan-headed screws which rigidly attach the prism-box to the collimator section. The circular wall and top of the box are of brass, and all are fastened in place by screws.

For the purpose of adapting the spectrograph to work in any part of the spectrum, the prisms are mounted with a device for keeping them automatically in the position of minimum deviation. The framework of the device ends in a rectangular box, the outer end of which receives the camera tube. The sides of the box, toward the inner end, are cut away for some distance. This allows the top and bottom to pass above and beneath the third prism, where they are securely clamped, respectively to the top and base of the prism-box. Milled-head screws passing through the cover of the prism-box press lightly on the mountings of the prisms, holding them in position against the base. The box, complete with the prisms, weighs 21½ pounds.

The camera tube screws into the prism-box mechanism, with the lens near the third prism. Its upper end is held in place by two rods which extend from the web of the collimator section and clamp to the tube. The instrument is supplied with two cameras, 15 and 19 inches long, respectively. Their tubes are of brass, 2½ inches in diameter and are supplied with focusing racks and millimeter scales. The long camera is equipped with a plate-holder receiver arranged for tipping the photographic plate, and with slides for occulting parts of the spectrum that tend to become overexposed. The plate-holders, four in number, are of brass and hold plates of size 2 by 3 inches. An eyepiece made to fit in the camera instead of the plate-holder is found extremely useful for viewing the spectrum to test the

performance of the spark, and to judge the exposure-time for the star as well as to test the guiding. It is kept in reach, and when the plate-holder is withdrawn it is slid into place and left until the next exposure is to begin.

Two guiding devices are provided. One utilizes the light stopped by the jaws of the slit; the other, the light which is reflected from the face of the first prism. The highly polished speculum slit-jaws are sufficiently inclined in the same plane to return the reflected light just outside the cone of rays from the large object-glass. A totally reflecting prism intercepts the reflected beam about 6 inches above the slit and directs it into a guiding telescope which stands at right angles to the optical axis of the refractor and is mounted on the arm extending from the upper end-casting of the collimator truss. The field of this guiding telescope is such that it includes the star when it has been brought into the center of the field of the 4-inch finder, and the star is quickly set on the slit by turning the slow motions. In the guiding tube one sees the image of the star or planet with the slit crossing it as a dark line. The center of the slit is marked by drawing the occulting tongue until its point is nearly in contact with the dark image of the slit. This pointer and a reticle in the tube enable one to guide very satisfactorily. Accurate guiding is more necessary in planetary than in stellar work, for it is generally required that the light entering the slit should come only from some particular part of the planet's disk. It is in this work that this method, first used by Huggins, is practically indispensable.

By the other method, due to Vogel, the light reflected from the first face of the first prism passes directly into a guiding tube mounted on the wall of the prism-box. The tube is constructed with an elbow so that the observer may rotate the eye-end into a convenient position for guiding. Although the guiding is done principally by the Huggins device, this one is always used to time the exposure when the spark spectrum is photographing, and, by removing the eyepiece, to see whether the whole of the collimator lens is illuminated by the spark.

The sparking apparatus is mounted on the arm which carries the Huggins guiding tube. It is supplied with a device for holding the metallic electrodes, which is readily replaceable by one carrying vacuum tubes. A condensing lens of wide field is mounted between

the spark and the slit-plate. It throws an image of the spark on a totally reflecting prism which directs the light into the collimator. The spark-gap for the tubes and the electrodes is parallel to the collimation axis.

A large induction coil capable of a 12-inch spark in air gives the high potential current for the spark. It receives current from a 104-volt alternating current with 133 complete oscillations per second and of about one unit amperage. Three one-gallon Leyden jars in the secondary circuit serve to suppress the air spectrum. A rheostat in the primary circuit is used to adjust the current to suit the spark-gap so as to give a clear metallic spectrum.

For protection against temperature changes the entire spectrograph, except the slit and guiding tubes, is inclosed in a wooden case. As Plate I shows, the case is wide at the top to permit work in the less refrangible parts of the spectrum. It is made in two sections which fasten together with hooks and eyes. A heavy clamp, half of which is firmly mounted on the upper end of each section, takes hold of one of the truss rods just above the web near the upper end of the collimator tube. This supports the entire weight, 22 pounds, of the case and holds it free from the spectrograph. The box is made of red cedar and lined throughout with layers of corrugated and felt paper covered with heavy silk plush. A hinged door over the end of the camera permits the spectrum to be observed and gives access to the plate-holder.

An electric heater containing 30 feet of No. 20 German silver wire is mounted in each half of the case, opposite and extending above the semicircular base and top of the prism-box. The heating current is led in from the mains of the 104-volt alternating current with which the dome is lighted. An incandescent light, with bulbs of different candle-power, is placed in series with the heaters, and a finer regulation of the quantity of current is effected by a twenty-one step rheostat, while a switch in reach of the observer starts and stops the heating current. Thus any desired quantity of current is at hand, and the maintenance of the prisms at a constant temperature is rendered easy.

A large-scale thermometer mounted with its cylindrical bulb along the base of the second prism records the temperature within the

prism-box. Its scale is a little greater than 1 mm to 0°.1 C., so that it can be read with ease to 0°.02. The temperature of the air-space within the wooden box is read from a second thermometer mounted opposite the first. Both are read through a window. The prism-box is additionally protected by three thicknesses of heavy woolen cloth. Thus a change of temperature shows itself in the outer thermometer in time to alter the current before the effect reaches the prisms. In general, the temperature does not change more than 0°.1 or 0°.2 C. during a night's observations, and it rarely happens that the variation is more than 0°.1 C. during an exposure of two or three hours.

After the spectroscope had been mounted and the adjustments had been made, in the spring of 1902, the first work undertaken was a determination of the color-curve of the 24-inch objective. The determination was made visually with the prism-train. Later, in the summer, the blue and violet part of the curve was redetermined photographically by use of a single dense prism. This gave the photographic part of the curve quite accurately and in close agreement with what the visual observations had made it. The color-curve is reproduced in Fig. 1, I. That part of it to the red of $H\beta$ depends upon the visual observations; the other end of the curve is principally from the photographic data. As may be seen from the curve, when the slit of the spectroscope was placed in the focus of the $H\gamma$ rays, it was about 20 mm inside the focus of the $H\delta$ rays and 19 mm outside the focus of the $H\beta$ rays. Thus the cones of the $H\delta$ and $H\beta$ rays from a star would have a diameter of about 1.2 mm where intersected by the slit-plate, so that the slit (0.02 mm wide) could admit only a very inconsiderable part of those rays, and the star spectrum would decrease in intensity quite rapidly at short distances on either side of $H\gamma$, particularly on the violet side, where the curve was steeper and where the absorption of those rays by the large objective and the lenses and prisms in the spectroscope entered to decrease the light intensity. Stellar plates, therefore, varied from overexposure in the center to weakness toward the ends, making them unsuitable for the most accurate measurement. In the case of the planets where there is a considerable disk, this decrease in the intensity of the spectrum was much less serious; *Jupiter's* spectrum, for example, extended the

full length of the plate (λ 4700 to $H\delta+$), though weak toward the violet.

It was evident that a distinct gain would result in all work if the color-curve were made flat through the photographic part of the spectrum. A copy of the curve was sent to Mr. Brashear. From

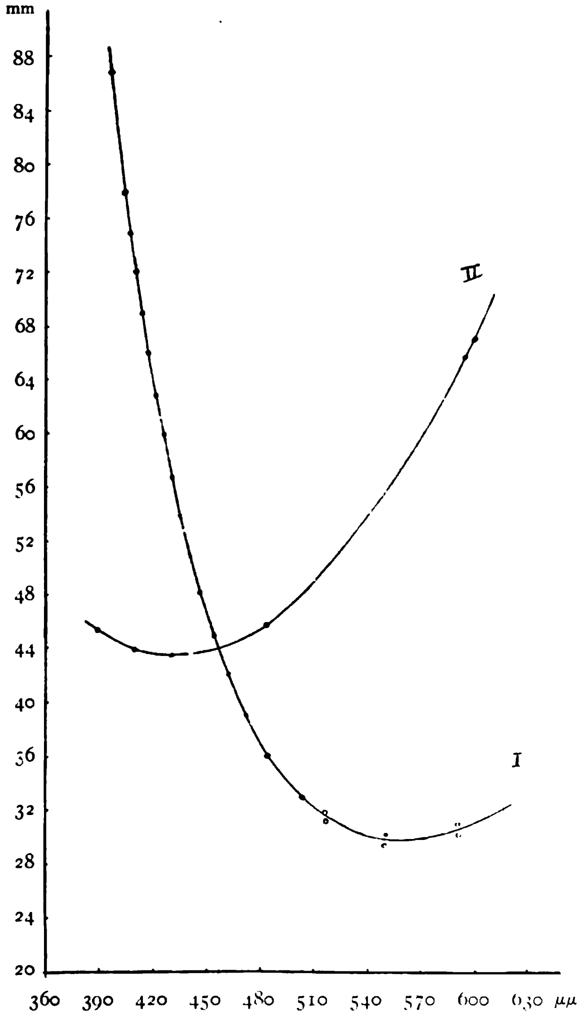


FIG. 1.—Color-Curve of 24-Inch Objective: I, without Correcting Lens; II, with Correcting Lens.

it Dr. C. S. Hastings computed a correcting lens which would unite upon the slit-plate the photographic rays as far on either side of λ 4384 as was feasible, for which ray the lens was not to change the angular aperture of the 24-inch glass. Since the planetary spectra in general fall off rather rapidly in intensity to the violet side of λ 4315, it seemed advantageous to make the central ray near λ 4400, and the correcting lens was constructed accordingly. The same decrease in brightness toward the violet is likewise true of the spectra of all solar-type stars; moreover, the most important lines for velocity work in the spectra of stars of the earlier types are comprised between $H\gamma$ and λ 4481, and consequently the choice of the central ray is equally advantageous in any stellar velocity work that might be undertaken.

Since the lens needed a somewhat larger undistorted field for planetary work than is required in stellar work, it was made larger than is usual. It has an aperture of 95 mm and is placed 1.422 m inside the focus of λ 4400, which light it brings to a focus about 8 mm nearer the object-glass. The lens is a doublet made of a double convex lens of a special light flint from Mantois, with very slight absorption, and a light crown lens, double concave in form. The two components are cemented together to avoid loss of light by reflection. The lens is mounted in a stiff tube 4 feet long, which slides by rack and pinion in a second tube having a threaded collar 5 inches in diameter which screws into the second joint of the tail-piece of the telescope in the place of the micrometer adapter. This gives the lens a stable mounting. The threads of this joint are "interrupted," and the lens and section of the tail-piece may be quickly interchanged.

The lens renders a star spectrum practically linear from $H\delta$ to λ 4700, *i. e.*, for 300 tenth-meters on either side of λ 4400. With it, the focus of $H\gamma$ is less than 1 mm shorter than that of $H\delta$ and about 2.5 mm shorter than that of $H\beta$. It transforms the color-curve of the 24-inch glass into II of Fig. 1.

The lenses of the spectrograph, four in number, are composed of crown and flint glass from Mantois, Paris. The construction, which is the same for all, is a doublet lately designed by Dr. Hastings, with the inner (contact) surfaces cemented.

There are two collimation lenses, each having an aperture of

30.5 mm. The one designed for work on the photographic rays is corrected for $H\gamma$; the other, to be used in the lower part of the spectrum, is correspondingly corrected.

The focal length of the former lens was determined in the laboratory. After the slit had been placed in the focus of the lens, by Schuster's method, and the short camera focused for parallel rays, the camera objective was directed into the collimator objective, and a negative of the spectrum of iron was pressed with its film against the jaws of the wide-open slit, illuminated, and its image in the camera photographed. Measurements under the microscope of the relative linear scales of the original spectrum and its photographed image gave the collimator a focal length 1.2683 times that of the camera, or 489.1 mm. When the photographic lens was replaced by the visual lens and the slit placed in its focus for the yellow rays, it was noted that the collimator scale-reading was increased 2.2 mm, and therefore that the focal length of the visual lens was 491.3 mm. Thus the angular aperture of the lenses is 1 to 16, the same as that of the 24-inch object-glass.

There are two camera lenses, each of 36 mm aperture, but differing considerably in focus. Their focal lengths were determined by star trails. The measurement of a plate of trails of the *Pleiades* gave the following values for the focal length of the long camera:

Star 16	*Tauri*	f = 471.4 mm
17	"	471.7
19	"	471.1
20	"	471.5
21	"	471.4
22	"	470.9
23	"	471.4
25	"	471.5
27	"	470.9
28	"	471.6
1170	*B. A. C.*	471.6
	Mean	f = 471.4 mm

This makes the angular aperture of the lens 1:13.

Measures on a plate of the same group of stars gave 385.7 mm for the focal length of the short camera. Its angular aperture is 1 : 11.

It has been found that, with the long camera, a greater length of spectrum is sharply defined if the plate is tipped to bring the violet

end nearer the lens. As the tube is provided with a device for tipping, this causes no inconvenience. This lens appears to perform somewhat better at about λ 4500 ($H\gamma$ in center of plate) than does the short camera, while the latter lens seems to define more sharply to the violet side of λ 4250 than does the long one.

For visual observations of the spectrum, two telescopes are provided, each of 33 mm aperture. The focal length of one, as determined by star trails, is 496.4 mm; of the other, 255 mm. An excellent little micrometer which may be used with either telescope meets all needs for visual measurements. The screw has a pitch of 0.5 mm and a head divided into 100 parts.

In ordering the glass for the prisms from Messrs. Schott & Co., Jena, Mr. Brashear gave special instructions for homogeneity and fine annealing. Consequently the glass was cast in a block from which the prisms were sawed so as to secure symmetrical annealing and as high a degree of homogeneity as possible. The glass is a dense silicate flint, the same brand, O 102, as that used in the new Potsdam spectrograph, No. III. This glass was selected on account of its high degree of transparency and freedom from color. The three prisms were given the same dimensions, but, as they are not very large and are almost free from absorption, this does not occasion any considerable loss of light. The bases are each 67 mm, and the average length of the path of the light through the glass is slightly more than 100 mm. The angles and the minimum deviations of the prisms were measured here a few months ago. The results of the measurements and the corresponding indices of refraction are given below.

TABLE I.

Prism I, angle = 62° 52′ 56″.
Prism II, an le = 62 52 12.
Prism III, angle = 62 52 31.

Line	3 D	n
$H\alpha$	165° 22′	1.64344
D_2	167 18	1.64803
$H\beta$	172 11	1.66208
λ4529	174 41	1.66903
$H\gamma$	176 20	1.67448
$H\delta$	179 14	1.68101
H	181 5	1.68680
K	181 38	1.68826

The average index for $H\gamma$ is 1.67448, and the total deviation is 176° 29′.

The measured angles and the indices of refraction of the prisms were found to be somewhat too small for the dispersion the instrument had been giving. The prisms were readjusted for minimum deviation, and the dispersion is now what it should be from theoretical considerations. It was understood that all adjustments had been carefully made and the prisms fixed in a permanent position in their box before the instrument left the maker's hands; and as an examination showed that the prisms had not been appreciably displaced in transportation, work was consequently begun with the instrument without testing the adjustment of the prisms for minimum deviation. Inasmuch as it had been my habit to test frequently the focus of the collimator lens, the small amount the prisms were out of the position of minimum deviation at $H\gamma$ could not have sensibly affected the spectrum otherwise than to somewhat increase the dispersion.

The prisms have not been examined individually, but the prism-train has been tested as a whole. The tests were made visually on the solar spectrum. While the writer observed the spectrum, an assistant occulted one-half the collimator lens or diaphragmed it down to half its aperture, so that the light was made to pass in turn through the half of the prisms near the refracting edge, through the half near the bases, and through the central half of the prisms and lenses. It was thus found that the definition of the spectrum was best when the diaphragm had been used, and less good for the half near the bases than for the half near the edges. No difference could be detected in the definition when a half of the prism-face was occulted at right angles to the refracting edge. Different collimating and observing lenses were used, so that the differences are due largely to causes in the prisms, probably to a lack of homogeneity in the prism-glass. But, unless all the optical parts are perfect, some such differences in the definition are to be expected.

It was noted also that a beam of light passing through the part near the edge or base of the prism train did not focus quite at the same point that it did when passing centrally through the lenses and prisms. This, too, seemed to indicate a lack of homogeneity in the

glass, which, however, may be in part, probably not wholly, attributed to the rapid and considerable rise in temperature of the prisms, during the observations, occasioned by opening the dome and removing the temperature control and the top of the prism-box and turning the instrument on the Sun. It is therefore probable that the glass is not quite homogeneous, but perhaps it is as nearly so as it is possible to cast it.

A comparison of the angular and linear dispersion, at $H\gamma$, of this and the other largest spectrographs is to be found in the accompanying table:

TABLE II.

Spectrograph	Dispersion			
	Focal Length of Camera		Linear, Tenth-Meters per mm	Angular, for one Tenth-Meter
Lowell	S	386	14.5	36'.8
Lowell	L	471	11.4	36.8
Mills (Lick)		406	12.6	40.5
Potsdam III	1	560	10.2	36.5
Potsdam III	2	410	13.8	36.5
Bruce (Yerkes)	A	440	10.7	42.8
Bruce (Yerkes)	B	607	7.9	42.8

Since the Bruce prism-train is set at minimum for λ 4480, where the dispersion is somewhat decreased, the four spectrographs have practically the same dispersive power.

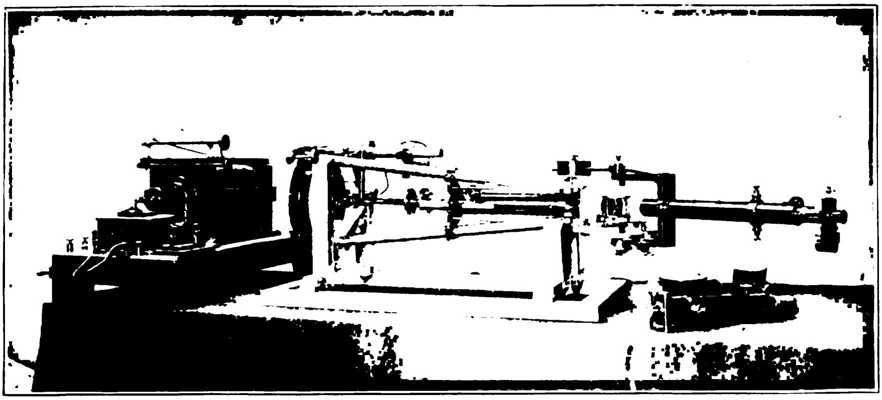
The linear dispersion for the two cameras at different points throughout the part of the spectrum employed at times in planetary and stellar radial velocity determinations with the Lowell instrument follows.

TABLE III.

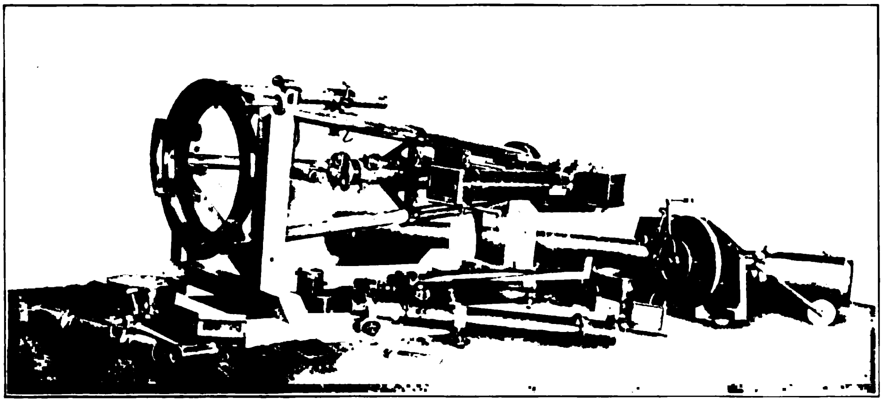
Line	Dispersion—Tenth-Meters per mm	
	Short Camera	Long Camera
$H\delta$	9.6	7.6
4200	11.4	9.1
4300	13.5	10.4
$H\gamma$	14.4	11.4
4400	15.7	12.4
4500	17.8	14.3

The limit of resolution of close lines on the sharpest negatives of

PLATE II.



A. The Spectrograph as Spectrometer.



B. The Spectrograph and Auxiliary Equipment.

the solar spectrum is about 0.11 tenth-meters at $H\delta$, 0.16 tenth-meters at λ 4300, and 0.22 tenth-meters at λ 4500. This is about the same as has been given for the other spectrographs. In the case of planetary and stellar spectrograms made on the most rapid plates—their grain being coarse—no such resolutions are possible; with such plates, and in the brightest stars, lines are not separated at $H\gamma$ which differ in wave-length by much less than 0.25 tenth-meters. In the case of faint stars, where the exposure time is longer and the slit-width greater, the practical resolving power is still less.

When it is desired to employ one of the single prisms or the grating the prism-box of the spectrograph is removed and the spectrometer section is mounted in its stead, as shown in Plate II A, which gives a view of the instrument arranged with the long-focus telescope for spectrometer work in the laboratory. The section is strongly constructed and carries a large position-circle accurately graduated to thirds of degrees and supplied with two verniers reading to half minutes. It has all necessary clamping and slow-motion screws. The single prism is mounted on the plate of the section by two screws which pass through the prism-mounting. The grating is similarly mounted. Metal boxes inclose the prism or grating.

If the single-prism equipment is to be used for photographing, the camera is inserted instead of the observing telescope, and the outer end of its tube is held by two rods which project from the web of the collimator section.

The grating is a Rowland plane grating of 14438 rulings to the inch.

There are two single prisms, one a very dense flint ,the other a light crown. Their angles, deviations, and indices of refraction are given below.

TABLE IV.

FLINT PRISM.

(Angle = 59° 59′ 50″.)

Line	Deviation	n
$H\delta$	60° 23′.8	1.7360
$H\gamma$	59 49.4	1.7310
$H\beta$	57 48 6	1.7131
D_2	55 50.8	1.6954
$H\alpha$	55 21.1	1.6905

CROWN PRISM.

(Angle = 59° 59′ 30″.)

$H\delta$	46° 58′.8	1.6076
$H\gamma$	46 37.2	1.6038
$H\beta$	45 50.0	1.5955
D_2	44 53 0	1.5855
$H\alpha$	44 30.7	1.5815

The dispersion at $H\gamma$ for the two cameras is, for the flint prism:

Long camera, 32 tenth-meters per mm,
Short camera, 39 tenth-meters per mm;

and for the crown prism:

Long camera, 62 tenth-meters per mm,
Short camera, 76 tenth-meters per mm.

It is possible, therefore, to select from the prism-train, these two single prisms, and the two cameras that dispersion which will deal most advantageously with the light of the star.

The microscope used in measuring the plates is modeled after the one described and illustrated in Frost's Scheiner's *Astronomical Spectroscopy*. As it was constructed with special reference to planetary work, it has a position-circle and a vernier reading to single minutes. Thus the planetary plates may be measured for displacement due to axial rotation either directly by finding the difference for settings on the two ends of the Fraunhofer lines corresponding to the ends of the planet's equator, or indirectly by measuring the inclinations of those lines. The indirect or inclination method has proved to be the more accurate and has been employed in all the measures for rotation.

This observatory stands 7250 feet (2210 m) above sea-level, where the barometric pressure is reduced by nearly one-fourth, so that there is a considerable decrease in the absorption by the Earth's atmosphere. This is a great gain in spectroscopic work of all kinds, but particularly so in planetary work, since it is necessary to observe some of the planets at low altitudes. Diminution in absorption, which shortens the exposure time, is equivalent to increasing the light-power of the equipment.

The following data got from the spectrogram record book will give an idea of the exposure times. The $H\gamma$ region of the spectrum

of *Venus* has been photographed on the most rapid plates in one minute with a slit-width 0.02 mm. Under the same conditions, plates have been made of *Jupiter* with exposures of 18 minutes; *Mars*, in about 25 minutes; and *Saturn*, in 2½ hours. The spectrum of *Sirius* photographs, under the best conditions, in 1 minute with a slit-width of 0.02 mm. Under the same conditions, β *Orionis* requires about 4 minutes; *Vega*, 5 minutes; *Arcturus*, about 8 minutes; *Aldebaran*, about 20 minutes; and *Polaris*, 30 minutes. With a slit-width of 0.02 mm, which is the width generally used, and under average conditions, a second-magnitude solar-type star is given an exposure of 40 or 45 minutes, and an *Orion* or Sirian type star of the same magnitude, about 15 minutes. These exposures are for the short camera. With the long camera they are increased by about one-fourth.

The comparison spectrum employed in the velocity work with both planets and stars has usually been that of the iron spark. The spectrum of the hydrogen tube has also been used in some cases for stars of the early spectral types. On the plates of the planets made for investigating the selective absorption due to their atmospheres, the spectrum of the Moon has, whenever possible, been photographed, because it offers a direct comparison between the planetary and the (normal) solar spectrum.

About fourteen hundred photographs of spectra have been made, mostly with the prism-train. Besides plates of artificial light-sources in the laboratory, this number includes negatives of the spectra of stars, Sun, Moon, and of all the planets of the solar system and of the asteroid *Vesta*.

The plates made in the laboratory and those of the Sun and Moon were principally for adjustment and test purposes, and for the reduction of the measurements of the planetary and stellar plates.

Although the spectrograph is devoted primarily to work on the planets, and stellar work is undertaken only as the time permits, about six hundred stellar spectrograms have already been obtained for the determination of velocities in the line of sight. These are mostly of southern stars—those not so convenient for observation at the more northern observatories. They are principally of stars of the earlier spectral types.

The spectra of the planets have been photographed from D to $H\delta$. Plates have been secured of all the planets except *Neptune* with the dispersion of the prism-train. The faintness of *Neptune* (8.5 magnitude) and the character of its spectrum make it a difficult object and permit the use of the dispersion of only a single prism. The plates of this planet are of more than ordinary interest on account of the light they throw on the nature and constitution of its atmosphere. The same may be said of the plates of the visual end of the spectrum of *Uranus*. The results of the investigation on the spectra of the planets, with reference to their atmospheres, will be published in due time.

Of the series of spectrograms made for investigating the rotation periods of the planets, the sets of *Venus* and *Mars* have been measured and the results published.[1] The set of *Mars* was made for the purpose of testing the efficiency of the spectrograph for investigating planetary rotation periods, and for this reason is referred to here. The velocity of a point on the equator of *Mars* due to axial rotation amounts to only 0.24 km per second. The slit of the spectrograph was placed upon the planet's equator, so that the observed spectrographic velocity would be four times the limb-velocity reduced for what the planet is out of opposition and for the tilt of the equator to the line of sight, or, in the present case, amounting to about 0.90 km per second. The displacements of the planetary lines were determined by measuring the amount they were inclined to the lines in the comparison spectra. Some of the plates were made with the camera above the collimator and some with it below, so that on some the lines are inclined to the right, on others to the left, under the microscope; and the measurements were made without knowing the position of the camera, and consequently without knowledge of the direction in which the lines should be inclined. Seven plates were so measured. The measurements gave the inclination in the proper direction for six out of the seven plates. The unweighted mean of all gave a point on the equator of *Mars* a velocity of 0.21 km, or 0.03 km too small; and a rotation period of 28.3 hours instead of 24.6 hours.

In this connection are given the measures and reduction of a plate of *Jupiter*. A direct enlargement from this spectrogram is reproduced in Plate III. The photograph was made November 21^{d} 16^{h}

[1] *Lowell Observatory Bulletins* 3 and 4, and *Astronomische Nachrichten*, 3891–2.

G. M. T., 1903. The readings of the position angle of the microscope for the planetary lines and comparison lines are denoted by ϕ and ϕ_0, respectively. The spectrographic velocity

$$v_s = 4v\,(1 + \cos\alpha)\cos B = V_s\, w\, D \tan \Delta\phi$$

where v is the limb-velocity, α the angle between the Sun and Earth seen from *Jupiter*, B the jovicentric latitude of the Earth, V_s the wave-frequency, w the computed width of spectrum on the plate, D the linear dispersion, and $\Delta\phi$ the inclination of the lines.

TABLE V.

Line	Position Angle for: Planetary Lines	Position Angle for: Comparison Lines	Inclination of Planetary Lines	Dispersion Tenth-Meters per mm	Velocity of Limb: Observed	Velocity of Limb: O.−C.
λ	ϕ	ϕ_0	$\Delta\phi$	D	Observed	O.−C.
4171.1	9° 25′.0	......	2° 19′.2	7.09	12.13	−.36
72.8	9 24.5	......	2 18.7	8.02	12.12	−.37
73.6	9 28.0	......	2 22.2	8.03	12.44	−.05
82.0	......	7° 4.5	......	....	....	....
87.3	9 23.0	7 6.0	2 17.2	8.23	12.26	−.23
88.0	9 24.0	7 6.0	2 18.2	8.24	12.36	−.13
99.3	9 26.0	7 7.0	2 20.2	8.40	12.76	+.27
4202.2	......	7 5.0	......	....	....	....
06.7	9 24.0	......	2 18.2	8.51	12.72	+.23
15.6	9 17.5	......	2 11.7	8.64	12.28	−.21
19.5	9 23.0	......	2 17.2	8.70	12.87	+.38
22.4	9 23.0	......	2 17.2	8.75	12.92	+.43
27.5	......	7 4.5	......	....	....	....
33.7	9 22.0	......	2 16.2	8.92	13.06	+.57
35.4	9 16.5	......	2 10.7	8.94	12.55	+.06
36.1	......	7 6.0.	......	....	....	....
39.0	9 16.0	......	2 10.2	9.00	12.57	+.08
40.0	9 13.0	......	2 7.2	9.01	12.29	−.20
43.5	9 18.0	......	2 12.7	9.07	12.89	+.40
45.5	9 15.5	......	2 9.7	9.10	12.64	+.15
47.0	9 19.5	......	2 13.7	9.12	13.06	+.57
50.3	9 16.5	7 7.0	2 10.7	9.17	12.82	+.33
51.0	9 9.5	7 8.0	2 3.7	9.18	12.15	−.34
54.5	9 10.0	......	2 4.2	9.23	12.26	−.23
75.0	9 14.5	......	2 8.7	9.5	13.08	+.59
82.6	......	7 5.0	......	....	....	....
93.3	9 5.0	......	1 59.2	9.84	12.42	−.07
94.3	9 9.0	7 4.5	2 3.2	9.86	12.86	+.37
98.2	9 8.0	......	2 2.2	9.92	12.83	+.34
99.4	......	7 6.0	......	....	....	....
4301.6	9 4.5	......	1 58.7	9.97	12.52	+.03
07.0	9 10.5	......	2 4.7	10.06	13.24	+.75
14.3	9 3.0	......	1 57.2	10.19	12.58	+.09
15.2	8 59.5	7 5.5	1 53.7	10.19	12.21	−.28
4318.8	9 7.5	......	2 1.7	10.26	13.14	+.65

Mean $\phi_0 = 7°\ 5'.8$ Mean velocity of limb = 12.62 km

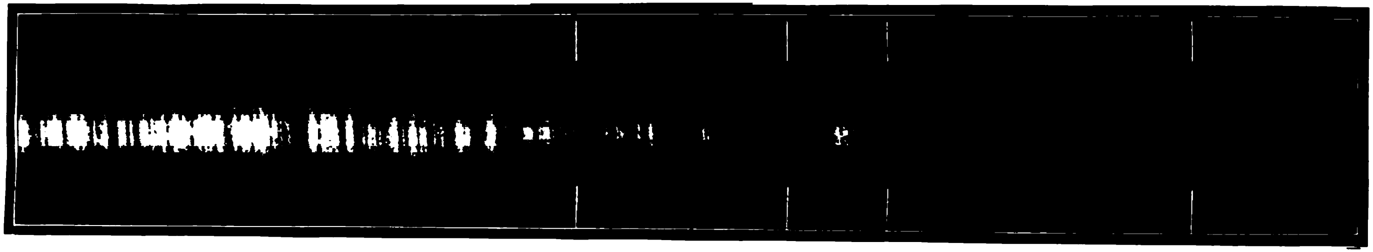
This plate gives the velocity of *Jupiter's* limb 0.13 km too large and the corresponding period of rotation ($9^h\ 50^m$) only five minutes too short.[1]

The results of the tests on the rotation of *Mars* and *Jupiter* give evidence of the satisfactory performance of the spectrograph in this line of work. A later paper will give some results of stellar radial velocity determinations made with this instrument.

LOWELL OBSERVATORY,
Flagstaff, Arizona,
April 1904.

[1] In computing the limb-velocity, the equatorial diameter of *Jupiter* was taken to be 141,940 km, and the mean period, $9^h\ 55^m$, was, by mistake, used. This period is, of course, not applicable to the planet's equator; for as A. Stanley Williams shows in *Monthly Notices*, **60**, 465, 1900, observers of *Jupiter* agree on a period of $9^h\ 50^m.4$ for the planet's equatorial zone. This period gives a limb-velocity of 12.63 km, with which the observed value 12.62 km is in excellent agreement.

PLATE III.



SPECTROGRAM OF *JUPITER* SHOWING ROTATION.

A DETAILED STUDY OF THE LINE SPECTRUM OF COPPER.

By A. S. King.

It is well known to every student of spectroscopy that the line spectrum of a metal is not invariable. It was early found necessary to distinguish between arc and spark spectra; and as our knowledge has advanced, it has become more generally recognized that a spectrum cannot be dealt with as a whole, but that each single line must be considered by itself. The same conditions of producing a spectrum may strengthen some lines, weaken others, broaden still others, etc., as compared with the spectrum produced by other conditions. So with copper we cannot speak of *the* line spectrum, but only of *a* spectrum which appears under certain given conditions. There is not only a spark spectrum and an arc spectrum, but an infinite variety of spectra, which often pass into one another by change of the manner of production.

Concerning the origin of vibrations, the mechanics of luminosity, we know as yet very little, in spite of numerous hypotheses. However, we have reason to hope that by close study of the conditions for the appearance of single lines we may arrive at some conclusions concerning these fundamental questions of spectroscopy. If we can show, for example, that some lines appear only with increased temperature, others with greater vapor density, and still others only when certain electrical oscillations are present, we shall have made a step in the desired direction. Clearly the problem is not to be solved by one person or in a few years. The conditions for the production of spectra are so extremely complicated, since by the change of one factor—as, for example, the current strength—so many other elements are at the same time altered that it is very difficult to refer an observed change in the spectrum to one definite cause. We can hope to grasp the true meaning of phenomena only through accumulated observations, given by different observers attacking the question from different points of view, and the investigation of numerous elements.

My study of the copper spectrum is thus to be regarded only as a beginning in this difficult field.

A detailed investigation of a single element is of importance in another direction. The statement that each line shows a special behavior does not apply to lines belonging to a series. Such lines are without doubt emitted by a single vibrating particle, and must therefore all change together. If, from among the lines not belonging to a known series, we can separate groups of lines which always change in the same way, we are justified in considering them as given by one and the same vibrating particle. Regularities in arrangement may then be looked for among the lines of such groups, and so the problem of luminosity is attacked from another side.

The study of all possible variations in the spectrum of a single element is a task of indefinite magnitude, and no completeness can be claimed for the work on the copper spectrum here presented; but the arc and spark spectra of copper have been studied as given by a large number of the most typical and diverse conditions, whose effects permit some conclusions to be drawn as to the way in which these conditions act.

Copper is a favorable element for an investigation of this kind on account of having numerous lines sensitive to change in physical conditions, and the arc between copper terminals can be controlled to a degree unusual among metallic arcs. The arc burns uniformly through a wide range of current, and without the violent combustion and rapid oxidation which with some metals prevent the accurate measurement of current and timing of exposures. With the spark spectrum, while here again the conditions of the spark can be readily controlled, a disturbing element is the presence of the strong air spectrum given by the condensed spark, which conceals a number of characteristic copper lines.

APPARATUS AND METHODS.

The voltage for the arc circuit gave a range from 36 to 440 volts by means of 90 storage cells and dynamo circuits of 220 and 440 volts.

For the spark, three induction coils were used, the largest being a Klingenfuss inductor giving a spark 1 m long, and allowing for a

variation of electromotive force by the use of different numbers of turns in the primary. Another coil gave a spark of 25 cm, but of considerable thickness; while a third coil gave but a 10 cm spark.

Leyden jars of 0.0027 henry each were used for capacity, and an inductance spool of 0.003 microfarad to observe the effect of self-induction. When gases were used around the spark, it was inclosed in a vertical glass tube through which the gases were passed, there being a side tube opposite the spark closed by a quartz window.

The spectrum was photographed by means of a concave grating of 1 m radius, adjusted to photograph the region from λ 2200 to λ 5800.

Besides the precautions necessary in manipulating the arc and spark to secure the desired conditions in each case, special care was required to avoid over-exposure, which tended to bring the weak and diffuse copper lines to the same intensity as the strong, sharp lines, these last being but slowly enhanced by longer exposures.

In comparing the intensities of lines, standard conditions were chosen for the arc and spark: for the arc a photograph taken with 220 volts and 5 amperes, and for the spark the spectrum as given by the spark in hydrogen, the air lines being absent. The intensities in each photograph were graded by means of a scale described in a previous article,[1] the appearance as well as the blackness of the lines being considered. The other photographs of arc and spark were th n compared line for line with these standard conditions, the scale being used to decide the amount of difference between corresponding lines.

The effort was made to secure comparison photographs of about the same average intensity. To equalize the effects of different exposure and development affecting the spectrum as a whole, it was found best to make one line of equal intensity for all the different conditions. In most of the photographs the line λ 4651 was but slightly changed, so after the intensities for each kind of arc or spark had been tabulated, as compared with the standard condition, this line was given the same value in each, and the values for the other lines in each case changed in the same proportion.

[1] A. S. King, Astrophysical Journal, **19**, 225–238, May 1904.

The experiments with the arc included the use of voltages of 36, 72, 108, 180, 220, 440 volts, with a series of different current strengths for each, and also copper in the carbon arc. With 72 volts a maximum of 54 amperes was reached. With 180, 220, and 440 volts a current of 0.5 ampere would maintain a steady, though very small, arc; but lower than this the arc was constantly broken. For a still lower current, the arc was made in a succession of flashes, the arc breaking as soon as the terminals were separated, an hour or more of the constant interruptions being required to obtain a photograph. In these flashes the current was of course constantly changing, but did not rise above 0.3 ampere.

With the spark, a series of photographs was made with varying electromotive force and capacity, with the use of self-induction, hot electrodes, varying spark-length, and gap in series, as well as the spark in hydrogen and oxygen with and without an outside gap, and a study of the different regions of the spark as projected on the slit.

I give below only a few tables out of the material collected. These show some of the most diverse conditions of arc and spark, with the effects on the stronger copper lines. Besides these, I have made an extended comparison of the other arc and spark conditions, and the grouping of lines resulting from this will be treated in the discussion.

EXPLANATION OF TABLES.

Column 1: intensities of arc lines given by arc between copper rods, with dynamo current of 5 amperes and 220 volts.

Column 2: arc burning continuously with 0.5 ampere and 220 volts.

Column 3: interrupted arc of 0.3 ampere (maximum) and 440 volts.

Columns 4–7: spark spectra from large inductor with 10–12 amperes primary current and 4 mm spark-length. Column 4: condensed spark with 0.0135 microfarad capacity. Column 5: same capacity and with inductance of 0.003 henry in circuit. Column 6: with copper wires so thin as to glow and slowly melt, 0.0081 microfarad capacity being used. Column 7: spark in hydrogen with same capacity as (4) and (5).

The small dispersion renders it difficult to say much regarding the appearance of lines, and only some of the more notable characteristics are entered in the column of remarks. The mark (?) occurring frequently in the tables of spark spectra indicates that the intensity of the line is rendered uncertain by the superposed air or nitrogen spectrum. In the case of self-induction, such lines are in general weak, since they are concealed by the fine lines of the nitrogen bands. The Roman numerals refer to the grouping of spark lines which will be spoken of later.

Group	λ	1 5 Amp.	2 0.5 Amp.	3 0.3 Amp.	4 Condensed Spark	5 Self-Induction	6 Hot Electrodes	7 Hydrogen	Remarks.
I	2242.68	..	..	..	5	¾	3	3½	
I	2247.14	..	..	..	5	¾	3	3½	
I	2276.30	..	..	..	5	¾	3	3½	
	2293.92	2½	½	½	..	..	..	..	
I	2369.97	2	½	2	7	4	6	5	
	2392.71	2	½	0	..	..	..	..	
	2441.72	2	1	1	..	..	..	..	
III	2494.97	2	1½	1½	2½	3½	2	½	
I	2506.50	..	..	..	5	½	2	3½	
I	2529.60	..	..	..	2½	½	½	1½	
I	2545.08	..	..	..	7	1½	3	5	
I	2600.51	..	..	..	6	½	1	3½	
III	2618.46	6	4½	3	6	4	6	3½	
I	2666.61	..	..	..	2½	0	0	1½	
I	2689.66	..	..	..	7	¾	2	5	
I	2701.34	..	..	..	7	¾	2	5	
I	2703.48	..	..	..	5	½	1	3½	
I	2713.82	..	..	..	7	¾	2	5	
I	2719.14	..	..	..	5	½	1	3½	
I	2721.98	..	..	..	2½	0	½	1½	
III	2766.50	5	3½	2½	5	5	6	3½	
I	2769.88	..	..	..	10	1½	3	7	
III	2824.47	6	5	5	10	7	8	5	
I	2837.66	..	..	..	5	0	0	3½	
I	2878.02	..	..	..	5	½	1	3½	
III	2883.03	2½	2½	1½	2	3½	4	¾	
III	2961.25	7	6	6	10	8½	8	5	
III	2997.46	2½	2½	2	3½	3½	3	1½	
III	3010.92	4½	4½	4	7	6	5	3½	
III	3036.17	4½	4½	4	7	6	5	3½	
III	3063.50	4½	4½	4	7	6	5	3½	
	3073.89	2½	2½	2	..	..	..	..	
	3094.07	3	3	2½	..	..	..	..	
I	3099.97	2½	1	1½	6	?	3	3½	In (6) strongest at poles.
I	3108.64	4	1	2	8½	?	5	5	In (6) strongest at poles.
	3116.48	2	0	½	..	..	..	..	
I	3126.22	3½	1	2½	8½	?	5	5	In (6) strongest at poles.
I	3128.73	2½	0	1	3½	?	2	1½	
I	3140.42	2	0	1	2½	?	1	1½	
I	3142.47	3	½	1½	5	?	4	1½	
I	3146.93	2	½	1	2½	?	2	1½	
I	3169.73	2	½	1	3½	?	1	1½	
III	3194.17	4½	4½	4	7	6	5	3½	
II	3208.32	3½	3½	3	5	1½	3	1½	
I	3235.74	..	..	..	5	?	2	3½	
I	3243.21	..	..	..	7	?	4	5	
II	3247.65	40	34	28	27	21	27	16	Revised in (1).
II	3274.06	35	30	25	24	19	24	14	Revised in (1).
III	3279.89	3	2½	2½	5	?	4	3½	
I	3282.78	2	0	1	7	?	2	5	In (6) strongest at poles.
I	3290.62	3½	1½	2	10	3½	5	7	In (6) strongest at poles.

Group	λ	5 Amp. 1	0.5 Amp. 2	0.3 Amp. 3	Condensed Spark 4	Self-Induction 5	Hot Electrodes 6	Hydrogen 7	Remarks.
I	3308.10	5	3	4½	12	7	7	8½	In (6) strongest at poles.
I	3317.28	1½	1	1	6	?	2	3½	
III	3337.95	4½	5	3½	6	6	4	3½	
I	3349.38	1½	0	½	2½	?	1	1½	
I	3365.46	3	½	1	7	?	3	5	In (6) strongest at poles.
I	81.52	2	0	½	5	3½	1	3½	
	384.88	1	2½	2	..	..	..	..	
I	3450.47	3½	½	1½	8½	?	2	5	In (6) strongest at poles.
I	3454.76	2	2½	1	2½	?	½	1½	
I	3476.07	3	½	2	5	?	1	3½	In (6) strongest at poles.
I	3483.84	3½	½	2	8½	?	2	5	In (6) strongest at poles.
I	3512.19	4	1	2	5	?	3	4	In (6) strongest at poles.
I	3520.07	2	0	1	5	?	2	3½	
I	3524.31	3	½	2½	7	5	3	5	
I	3527.55	2	0	½	2½	?	2	3½	
III	3530.50	3½	4	3	5	?	4	1½	
I	3533.84	2	0	½	5	?	2	3½	
I	3599.20	5	2½	5	10	4	5	7	In (6) strongest at poles.
I	3602.11	5	2½	5	10	4	5	7	In (6) strongest at poles.
I	3613.86	2½	½	2	5	?	2	3½	
I	3621.33	2	0	1½	5	?	2	3½	
	3641.79	2	4	1	..	..	..	..	
	3654.60	4	2½	1	..	..	..	..	Broadened toward red.
	3684.75	3	5	2½	..	..	..	..	
I	3687.75	..	..	..	7	?	2	3½	
	3688.60	7	3½	1½	..	..	..	..	Broadened toward red.
	3700.63	2	0	1	..	..	..	..	
	3741.32	2	0	1½	..	..	..	..	
	771.96	1½	0	½	..	..	..	..	
	3805.33	1½	0	1½	.	..	..	..	
	3821.01	2	?	1½	..	..	..	..	
	3825.13	3	1½	1	..	..	..	..	
III	3860.64	3½	3½	3	2½	?	4	3½	
	3861.88	4	2	2	..	..	..	..	
	4015.80	1½	½	1	..	..	..	..	
III	4022.83	13	10	10	10	8½	14	7	Broadened toward red.
I	4043.70	..	..	..	?	3½	0	5	
	4056.80		8	4	..	..	..	..	
II	4062.94	15	12	12	12	10	18	8½	Broadened toward red.
	4123.33	2½	0	1	..	..	..	..	Diffuse
II	4177.87	4	½	2	?	3½	4	3½	Diffuse.
	4242.42	2	0	1	..	..	..	..	Diffuse.
III	4249.21	5	3	6	5	5	6	5	
	4259.03	4	½	3	..	..	..	..	Diffuse.
II	4275.32	10	9	12	15	8	14	10	
II	4378.40	9	3	8	10	6	10	7	Diffuse. In (5) strongest at poles.
I	4415.79	5	1	4	10	3½	4	3½	Diffuse. In (5) strongest at poles.
III	4480.59	8	8	8	5	5	6	3½	
I	4507.02	6	0	4	8	2	3	4	Diffuse. In (5) and (6) strongest at poles.
II	4509.60	7	6	10	10	5	7	7	In (5) and (6) strongest at poles.

Group	λ	1 5 Amp.	2 0.5 Amp.	3 0.3 Amp.	4 Condensed Spark	5 Self-Induction	6 Hot Electrodes	7 Hydrogen	Remarks
III	4531.04	9	9	9	?	8½	8	5	
II	4539.[illegible]8	8	1½	6	7	3½	6	7½	Diffuse. In (6) strongest at poles.
I	4555.94	..	..	..	5	1½	1	3½	
II	4587.19	9	3	8	10	5	8	8½	Diffuse. In (5) and (6) strongest at poles.
	4642.78	3	0	2	..	..	..	..	Diffuse.
III	4651.31	12	12	12	12	12	12	12	
	4674.94	5	½	3	..	..	..	..	Diffuse.
	4697.62	3	0	2	..	..	..	..	Diffuse.
III	4704.70	7	7	7	?	5	4	5	In (5) and (6) strongest at poles.
-	5076.42	2	1	¼	..	..	..	..	
II	5105.75	17	18	17	19	14	18	10	
II	5153.33	16	14	16	19	12	16	14	
II	5218.45	18	16	18	24	15	20	17	
II	5220.25	6	7	7	10	8½	8	7	
II	5292.75	7	6	9	12	7	6	7	In (5) and (6) strongest at poles.
	5391.89	3	0	1	..	..	..	..	Diffuse.
	5432.30	2	0	½	..	..	..	..	Diffuse.
	5536.06	3	0	1½	..	..	..	..	Diffuse.
	5555.16	2	1	3	..	..	..	..	
III	5646.93	6	5	4	10	8	8	5	
III	5700.39	12	13	12	13	12	14	6	
III	5782.30	15	16	15	17	15	18	8½	

THE ARC SPECTRUM.

1. *Effects of varying current.*—Comparing the three arc spectra tabulated, which I shall designate respectively as the strong, weak, and interrupted arcs, we can see how far the conclusions of Hartmann[1] hold for the copper spectrum. Hartmann found that in the spectra of magnesium, zinc, bismuth, and lead certain strong spark lines which are absent or very weak in the strong arc appear with considerable intensity in the arc with very small current, constantly broken. This result seemed to depend primarily upon the current being continually interrupted, which would be expected to produce strong oscillations in the arc, and to approach as near the spark conditions as the low electromotive force would allow.

The copper spectrum contains no lines on which the action of the weak arc is so decided as on the line λ 4481 of magnesium, for example. The strong arc gives distinctly all lines which appear in the arc under any circumstances. The interrupted arc does not

[1] ASTROPHYSICAL JOURNAL, **17**, 270, 1903.

bring out any lines which usually appear in the spark alone. There are, however, a number of lines in the spark spectrum which are greatly weakened by self-induction, hot electrodes, and other conditions which weaken the spark discharge. These may then be designated as "spark lines" in the sense that they are favored by the conditions of the strong spark. It will be seen from the tables that, as a rule, when these lines appear in the arc at all, they are much reduced by the weak continuous arc, but *restored to considerable intensity by the interrupted arc*, sometimes becoming relatively stronger than in the strong arc. Types of this class of lines are the following:

3108	3308	3484	3614
3126	3450	3599	3621
3290	3476	3602	4416

The evidence seems strong that the interruptions are responsible for this change. The intensities of these lines suffer, as it were, a discontinuity, the lines becoming gradually weaker with the decrease in current until the arc will no longer burn continuously; then with a current only slightly weaker, the lines suddenly regain a considerable relative intensity. They are for the most part diffuse lines, and their decrease in intensity would be explained by the reduced vapor density of the weak continuous arc, were it not that we should expect a still smaller vapor density in the interrupted arc. Of this last point, however, we cannot be certain. It is conceivable that the high voltage producing a flash which is at once extinguished may give a high vapor density for this very brief time, higher than the very small arc burning quietly with a slightly greater current. A reduced vapor density probably exists in those spark conditions which weaken these lines, and they are relatively enhanced by increased capacity, which, while it strengthens the electrical discharge, must increase the vapor density.

It would thus seem that we must admit the possibility of the dependence of this set of lines on vapor density, with the temperature changes which would accompany change of vapor density. It seems to the writer more reasonable, however, to attribute the lines to oscillations in the arc and spark—oscillations not necessarily so violent as those which excite the vibrations of the distinctly spark lines

which never appear in the arc. Oscillations are doubtless present to a greater or less extent in the strong arc, and are favored by its more or less irregular burning and the fact that inductance is present with a dynamo circuit, and capacity with a battery of storage cells. These oscillations are probably much reduced by the weak continuous arc, which burns more steadily and usually very silently. The interrupted arc, being constantly rekindled, should greatly favor the oscillations. In the spark spectrum the behavior of these lines bears out the view that they are dependent on the strength of the oscillations. They are much reduced by self-induction in the spark circuit, and slightly enhanced by increased capacity. In a long and powerful spark they are much weaker in the middle of the spark than close to the electrodes. Also in the spark between glowing electrodes, in which experiments indicate that the oscillations are much weaker, we find these lines reduced.

The arc lines in my table are those appearing with moderate current strength. With a current of 8 to 10 amperes a large number of diffuse lines appear, chiefly in the blue and green, and become rapidly stronger as the current is increased, their vibrations probably requiring the stimulus of the high vaporization. The arc lines given by Kayser and Runge[1] and not in my table belong to this class.

2. *Altered voltage and conductivity.*—While with the copper arc the variation of current strength proved to be the most effective way of altering the spectrum, other methods produced changes which will now be noticed. The best type of a change by altered voltage was given by comparing the spectrum with 1 ampere and 440 volts dynamo current with that given by a storage battery current of the same strength with 72 volts. A number of lines were changed relatively to other lines. Considering a few distinctive cases, the pairs λλ 3654, 3688, 4480, 4531 were enhanced by 440 volts relatively to the lines λλ 3684, 4509 adjacent to each pair. These pairs are changed in the same way by the use of inductive resistance in the otherwise non-inductive circuit of 90 storage cells, with 0.5 ampere and in addition we have now the pairs λλ 4023, 4063, 5153, 5218 5700, 5782 strengthened relatively to the adjacent lines λλ 4056,

[1] Anhang zu *Abhandlungen der Kgl. Akad. der Wiss. zu Berlin*, 1892.

5105, 5646. The changes are slight and may be the result of oscillations given by the inductance of the dynamo circuit and by the storage battery circuit with inductive resistance, as compared with the non-inductive circuits. These pairs are among the most distinctive arc lines. The "spark lines" considered above are too faint in these weak current arcs for comparison.

Copper in the carbon arc gives an arc of different conductivity from that between copper rods, because of the division of the current between carbon and metallic particles. A number of copper lines are much weakened, compared with an arc of the same current between copper terminals, chief among these being λλ 3365, 3381, 3450, 3483, 3599, 3602, 3613, 3621, 3654, 3688, 4023, 4063, 4249, 4378. These lines are in all cases shown by my table to be much reduced by the weak continuous arc, and generally restored to considerable intensity by the interrupted arc. The action of the copper-carbon arc suggests an explanation of this. In the first place, the density of copper vapor is probably much less than in the copper arc; and, secondly, the copper-carbon arc burns much more quietly and uniformly than the copper arc of equal current, suggesting a weakness of the oscillations which was noted as a probable property of the low current copper arc.

Nitrogen bands in the weak arc.—It has been several times noted in recent years that the negative-pole nitrogen bands sometimes appear in the carbon arc. In the copper arc spectrum, as given by 440 volts and 0.5 ampere, I have obtained very distinct nitrogen bands, not only the heads, but the whole structure of the bands, being clearly visible, almost as strong as in the spark spectrum with self-induction. The bands were visible, but not so strong with the weak interrupted arc of 440 volts. The conduction of a considerable part of the current by the nitrogen would be expected to weaken some lines more than others, and this division of the current may play some part in producing the changes observed with the weak arc. As regards the medium between the electrodes, the arc with weak current and high voltage thus appears to approach the spark discharge.

3. *Different regions of the arc.*—An important change was observed when the outer part of the middle of a long arc was projected on the

slit, and this photograph compared with one taken with the same current and same length of arc with the part next to the positive pole projected. A much longer exposure was needed for the outside of the arc, and the result gave a group of lines much reduced relatively to the others. When the lines λλ 3247, 3274, 3337, 3524, 5105, 5646, 5700, 5782 and the strong arc lines of shorter wave-length are brought to nearly equal intensities in the photographs by properly timed exposures, a large group of characteristic lines is much reduced. These lines are as follows:

3308	3688	4480	4704
3599	4023	4509	5153
3602	4063	4531	5218
3654	4275	4651	5220

In this latter group the line pairs λλ 3654, 3688, 4023, 4063, 4480, 4531, 5153, 5218 are reduced somewhat less than the strong unrelated lines λλ 4275, 4378, 4509, 4651, 4704.

The experiment corresponds to Lockyer's method of long and short lines with a prismatic spectrum.[1] It serves to separate the lines present in all parts of the arc from those given strongest by the central portion, where temperature and vapor density are at a maximum and the vapor purest. However, that temperature or vapor density is the controlling element for these lines is rendered doubtful by the fact that some of the lines most reduced by the outer layer of the arc, viz., λλ 4275, 4651, 4704, are reduced little, if at all, by the arc with very weak current. Lecoq observed them in a weak spark between electrodes containing copper chloride. Also, λ 5218, which is considerably reduced in the outer layer, appears in the Bunsen flame. The most plausible explanation seems to be that these lines are favored by (though not dependent upon) strong oscillations in the arc. These oscillations should be much weaker in the outside layer than in the middle next the pole. The facts that the lines are often strengthened in the interrupted arc, and in the spark are generally reduced by self-induction, accord with this view. Furthermore, the group which appear strong in the outside layer of the arc, and are to be regarded as the "persistent" lines of the spectrum, are as a rule little changed by altered spark discharge, especially the

[1] KAYSER, *Handbuch der Spectroscopie*, 2, p. 236.

strong arc lines below λ 3247, which show the least change in the outside layer of the arc.

De Watteville[1] has recently shown that with potassium in the coal-gas flame a series of zones exists, each of which gives off lines belonging to a particular series. So far as the known series relations allow, the copper lines show a similar behavior in the different parts of the arc: the strong ultra-violet pair appearing in the outermost layer of vapor, the pairs of the two subseries being then somewhat reduced, but not so much as the majority of the lines, which belong to the central part of the arc.

4. *Further notes on the arc.*—*a*) With a current slightly above 1 ampere, the relative intensity of lines is *independent of voltage.* Batteries of 36, 72, 108, and 180 volts were used, also dynamo voltages of 220 and 440, and were found to give identical spectra for the same current strength, except with very weak currents.

b) With a continuous arc the changes given by varying current are *always gradual.* This was determined by a series of photographs with different currents at each voltage. As an example, with low current λ 5153 is much weaker than λ 5105; the lines become equal at about 6 amperes, and λ 5153 becomes gradually stronger as the current rises.

c) *A change in length of arc*, with the same voltage, alters the spectrum only by changing the current. For example, the current may be reduced from 5 to 3 amperes by increasing the length of arc, and the relative intensities of some lines are thereby altered. The spectrum in the latter case, however, was found to be the same as that with the short arc when the current was reduced to 3 amperes by outside resistance.

THE SPARK SPECTRUM.

1. *Changes by altered discharge.*—The changes in the spark most effective in producing relative differences among the copper lines were found to be the introduction of self-induction, the use of electrodes so thin as to become glowing, the use of atmospheres other than air, and the comparison of the middle of a long and thick spark with the part next to the electrode. Surprisingly small differences were given

[1] *Comptes Rendus*, **138**, 346, 1904.

by a large change of capacity or of electromotive force. It appeared that a small capacity, with an inductor giving a spark only a few centimeters long, was sufficient to produce the regular spark spectrum; and a large battery of Leyden jars with a powerful induction coil, while greatly shortening the exposure required, produced only a slight intensification of those lines most characteristic of the condensed spark. A radical change in the character rather than in the strength of the discharge was required.

The first important result to be noted is the similarity of the spectra given by the use of self-induction, by hot electrodes, and by the middle of a long spark. While these spectra are not identical, a larger number of lines, especially in the ultra-violet, are much weakened in all three cases. These lines are given of maximum intensity by large capacity, and when the end of a long and powerful spark from the large inductor is projected on the slit, the middle of this same spark showing the lines very weak. This combination of effects seems to justify ascribing these lines to the oscillations in the spark. Those which are chiefly affected are the lines most characteristic of the condensed spark, most of them never appearing in the arc. The group (see table) from λ 2689 to λ 2719 are types of this class. The reduction of these lines makes the spark spectrum more like that of the arc, which in other metals has often been observed to be the effect given by self-induction and by glowing electrodes.

In my tabulation I have divided the spark lines of copper according to their behavior under various conditions into three groups, as indicated by the Roman numeral opposite each line. In Group I are placed those lines which occur only in the spark, and also some lines the behavior of which, in the spark, while they appear in the arc, indicates that they are especially favored by the conditions of the strong spark. The lines of this latter class are enhanced when a more powerful spark is used, or when the part of the spark next the electrode is projected on the slit. They are much reduced by self-induction, by hot electrodes, and usually by atmospheres of hydrogen and oxygen. With self-induction and hot electrodes they show stronger at the poles, while the more persistent lines extend uniformly across the spark. Unlike the lines which appear in the spark alone, these lines are given distinctly by the middle of the

long and thick spark. This peculiarity, and the fact that they occur in the arc, points to the conclusion that, while they are favored by strong oscillations, the lack of these may be made up for by a strong vaporization, such as is present in the thick spark, and (compared to the spark) in any kind of arc. In the arc, the tables show these lines to be almost without exception greatly reduced by the small continuous arc, and restored to almost, if not to full value, by the interrupted arc. It is in this sense, as has been noted, that the interrupted arc with very weak current strengthens the spark lines.

Groups II and III consist of lines common to arc and spark, but which are not easily assignable to any one condition as sole cause. The lines of Group II are much reduced by self-induction, but their behavior under other conditions does not justify placing them in Group I. They are reduced in most cases by the weak continuous arc and appear with considerable strength in the spark with hot electrodes, when there is probably strong vaporization. These facts point to vapor density as governing the intensity of these lines, but their origin is evidently complex. The pair $\lambda\lambda$ 3247, 3274 are in all cases the strongest lines of the spectrum, and are the only lines which I have been able to obtain with an uncondensed spark. They were noted by Hartley[1] with a 0.01 per cent. solution of copper salt in the spark.

Group III includes the remaining lines in the table and are little altered by self-induction, hot electrodes, or the middle of a long spark. They are also the lines least changed by the three arc conditions tabulated. In the spark they are quite generally reduced by hydrogen and oxygen.

The effect of a *spark-gap in series* was tried with the spark in air, hydrogen, and oxygen. The greater part of the energy being used in the long outside gap, the smaller spark was reduced in intensity; but when this was compensated for by longer exposures, the chief difference which appeared was a relative strengthening of the true spark lines which are absent in the arc. This should result from the greater violence of the discharge given with the long auxiliary spark.

Small changes in *length of spark* produced little or no relative

[1] *Phil. Trans.*, 175, II, 325, 1884.

change in the spectral lines. With a spark-length of 1 cm or more, however, as noted before, the lines most characteristic of the spark appeared much stronger near the electrodes than in the middle of the gap.

A comparison of the *different regions* of the spark at the same distance from the electrodes was made by projecting in the one case the center of the luminous track, and in the other case the outer green coating of the spark on the slit. The latter photograph required three times as long an exposure as the first for the same intensity of the copper spectrum. The air spectrum was then almost entirely eliminated, showing that it belongs entirely to the luminous track, but surprisingly few relative changes appeared among the copper lines. The pair $\lambda\lambda$ 4023, 4063 are considerably strengthened in the outer portion of the spark, and other arc lines show slight changes. The experiment seems to afford strong evidence that the electrical action, rather than temperature or vapor density, is the governing element in the production of the spark spectrum. The electrical conditions should be nearly the same in the spark at the same distance from the electrodes, whether in the middle or outside of the spark, while the outer layer should have a lower temperature. The differences in the arc lines and the still larger changes previously noted when different regions of the arc were used may be explained if the arc lines are more dependent on temperature and vapor density, the properties in which the zones probably differ.

2. *Effects of atmospheres.*—The numerous changes in the character of the discharges by altered circuit conditions have in no case given a spectrum similar to that given by the spark in an atmosphere of hydrogen or oxygen. On this account it appears that the cause of the action of various gases is not to be looked for in changes of electromotive force, strength of discharges, period of oscillation, or temperature; but rather by the altered conductivity afforded by each gas. This would result in an altered division of the current between gas and metallic particles, and in this re-division it is highly probable that the electric stimulus needed for certain vibrations would be strengthened, and that for others weakened. The tabulation shows the numerous differences between the copper spectrum in hydrogen and in air. A large part of the current was evidently carried by the

gas, for although the bright red spark was as thick as in air, nearly ten times as long an exposure was required to bring the copper lines to intensity suitable for comparison. The hydrogen lines were very strong, $H\beta$ being reversed.

An atmosphere of pure oxygen, while not requiring so long an exposure as hydrogen, gave a very similar spectrum, but with relative differences in several important lines. With most of the lines of the same intensity with hydrogen and oxygen, λλ 4249, 4275, 4416 were given stronger in oxygen, and λλ 2618, 2766, 2837 were weakened. The oxygen spectrum was strong.

Naturally this altered conductivity is accompanied by an altered potential, but the changes given by a change of the conducting gas between the electrodes are different from the changes given by altered potential with the same gas.

A comparison of my table of intensities of the copper spark in hydrogen with that of Eder and Valenta[1] shows some relative differences between lines too large to be explained by different standards of intensity. For instance, the lines λλ 4275, 4378, given in my table as 10 and 7 respectively, are given by Eder and Valenta as 10 and 1. Also several lines, chiefly in the ultra-violet, which are very weak or absent in my photographs, are given by Eder and Valenta as of considerable intensity. This lack of agreement is doubtless largely due to the difference in the inductors used, and illustrates the difficulty of comparing intensity tables of spark spectra made by different observers by means of different apparatus. Further, it is quite possible that hydrogen, by reason of carrying so large a proportion of the current itself, makes the copper vapor more sensitive to changes in the character of the discharge, since I found that in air the copper lines are changed but slightly by the use of different inductors and capacities.

As another instance of the differences in spark spectra given by different kinds of discharge, I may cite the results of Demarçay[2] with copper-chloride solution. With his inductor having a secondary coil of thick wire, Demarçay obtains a spectrum greatly different from the usual spectrum of the spark in air, and approaching the arc spectrum, as indicated by the weakness of λ 5105 with respect to λ 4651, and the strength of the pair λλ 4023, 4063.

[1] *Denkschriften der Wiener Akad.*, **63**, 1896. [2] *Spectres électriques*, 1895.

It was expected that the introduction of a long outside spark-gap would reduce the differences given by hydrogen and oxygen around the short spark which was photographed, as the circuit conditions would be governed largely by the long gap in air which remained unchanged. The effects of the gases were not materially altered by this means, however, the only effect being a relative enhancement of some of the more distinctive spark lines, as was the case when an outside gap was used with the spark in air. This afforded additional evidence that it was the re-division of electricity between the two carriers, rather than an altered character of discharge, which produced the observed changes.

3. *The spark in water.*—Dr. Konen[1] found the arc spectrum of copper in water to be little changed from that in air. He has kindly furnished me with a photograph of the spectrum of the copper spark in water, taken in the course of his investigation. The strong continuous ground does not permit an accurate estimate of the intensities of the copper lines; but, so far as can be judged, the spectrum is but slightly changed from that of the condensed spark in air, except for the broad reversal of $\lambda\lambda$ 3247, 3274. One noteworthy difference is the weakness of λ 5105 compared to λ 5153. This appeared only in the spark spectra I have studied with atmospheres of hydrogen and oxygen, most decided in the case of hydrogen. This points to the hydrogen liberated by electrolysis as the cause of the slight change noted with the spark in water, which is in agreement with the conclusion of Hartmann and Eberhard,[2] that hydrogen is responsible for the alteration of arc spectra by water.

4. *Regularities and general structure of spectrum.*—In regard to the series relations, the rapid decrease in intensity of the successive members of the first subseries,[3] which in the arc spectrum renders the ultra-violet members invisible, is still more pronounced in the spark, the pair $\lambda\lambda$ 3654, 3688 not appearing, and the difference between the pairs $\lambda\lambda$ 4023, 4063 and $\lambda\lambda$ 5153, 5218 being greater than in the arc. In fact, the difference in intensity between corresponding lines of these pairs, as $\lambda\lambda$ 4023 and 5153, is a good indica-

[1] *Ann. der Phys.*, (4) **9**, 742, 1902.

[2] ASTROPHYSICAL JOURNAL, **17**, 229, 1903.

[3] See KAYSER, *Handbuch der Spectroscopie*, **2**, 530.

tor of the strength of the spark conditions, the difference being small with self-induction and hot electrodes, and very large next to the electrode with a long spark.

In addition to the known series, line pairs may frequently be selected which are changed in the same way by changed conditions. The arc pair λλ 4242.78, 4697.62, having approximately the same vibration-difference (251.4) as the series pairs, appears not to have been noted before. Some of the lines differing by numbers which Rydberg[1] found to be often repeated show similar behavior, but in many cases the different behavior of such lines indicates that they cannot be considered as *pairs* in the usual sense. As examples of such couples whose lines do not change in the same way, we have λλ 3126, 3194 and λλ 3063, 3129, each with the difference 681; also λλ 4651, 4697 and λλ 3614, 3642 with the difference 212, and others of the same sort. This dissimilar behavior, and the fact that a line was often found by Rydberg to be coupled with two or even three other lines by these frequently occurring differences, suggest that the differences arise from relations connecting the mathematical expressions, probably complex, for the vibrations of separate particles, rather than the grouping of lines which belong to the same particle.

Considering for a moment the copper spectrum as a whole, we see that strong spark discharges result in enriching the ultra-violet, the lines depending most on strength of discharge being in this region. The extreme arc conditions, given by a high current between copper rods, have their chief effect in the green and yellow, bringing out a large number of diffuse lines, which seem to require high vapor density. This relation between the effects of the two extreme conditions suggests a dependence on the principle, which is general in vibrating systems, that the vibrations of shorter period require a more violent stimulus—a stimulus which in this case is given by the powerful oscillations of the condensed spark.

CONCLUDING REMARKS.

The investigations of Schuster and Hemsalech[2] on the electrical processes in the spark gave us a picture of the manner of production

[1] ASTROPHYSICAL JOURNAL, **6**, 239, 1897.

[2] *Phil. Trans.*, **193**, A 189, 1889; also G. A. HEMSALECH, *Thèses*, Paris, 1901.

of the spectrum: the first discharge passing through the air, and the succeeding oscillations through the metallic vapor, the alterations in these oscillations, especially by the use of self-induction, changing the spectrum, enhancing some lines at the expense of others. Following this, the work of Schenck[1] showed that the spectral lines are dependent to different degrees on the oscillations, each line by his method showing the oscillations which produced it, and how far the intensity of the line was due to the electrical pulses and how far to the luminous vapor in the middle of the spark. His results showed that the distinctly spark lines give the oscillations much stronger than the lines which appear also in the arc. The action of self-induction in altering the period and character of the oscillations thus explains the fact that the lines most peculiar to the spark are most sensitive to the effects of self-induction, a reduction of these spark lines making the spark spectrum more like that of the arc, the arc lines depending least on the oscillations. Schenck and other observers have noted that glowing electrodes have an effect on the spark lines similar to that of self-induction. The heating of the medium makes it better conducting, and the electrical pulses are weakened. But with this increased conductivity comes a reduced potential, and probably considerable changes in temperature and vapor density. The change in division of the current between metallic particles and the gas between the electrodes, to which reference was made in the case of change of atmosphere, may have a considerable part in the changes given by self-induction and by hot electrodes. The spark between copper electrodes in air gives the air lines strong. This means that a large part of the energy of each spark is used up in producing the air spectrum and is not available to act on the copper vapor. With hot electrodes the air spectrum is much weakened, and the nitrogen bands appear faintly; while with self-induction the air lines have given way entirely to a strong nitrogen spectrum, doubtless in each case with a resulting change in the division of energy between metallic particles and gas. In the middle of a long spark we have a reduction in the strength of the oscillations, but a strong air spectrum and considerable vapor density.

My photographs show the copper spectra with self induction,

[1] ASTROPHYSICAL JOURNAL, 14, 116, 1901.

with hot electrodes, and in the middle of a long spark to be by no means identical, though having the common property of a reduction of the characteristic spark lines. The oscillations of the spark are weakened in each case, but the three conditions differ among themselves sufficiently to account for the differences in lines which probably do not owe their origin to any single state of the luminous vapor.

This work has been largely a study of the relations of various electrical conditions. The results for the spark spectrum show that the usual spark spectrum is given by a condensed spark of moderate intensity, and is very sensitive to changes which modify the character of the oscillations. As with other elements which have been studied from this point of view, a group of lines, chiefly in the ultra-violet, may be selected which appear to depend chiefly on the strength and frequency of the oscillations. Other lines depend to a less degree on electrical action, and others are of a still more complex origin. The chief changes in the arc spectrum were obtained by a variation of current strength, and by the weak interrupted arc. While the spectral changes in the arc can with some difficulty be explained by changes of vapor density, a comparison with the spark spectrum under various conditions, and a consideration of the oscillations in the arc, strong with high current, reduced by the weak continuous arc and given again strongly by the interrupted arc, appear to justify the conclusion that in the arc as well as in the spark the relative strength of a large number of copper lines depends upon the character of the oscillations present.

My hearty thanks are due to Professor Kayser for his constant interest, and for much advice given during the investigation.

University of Bonn,
April 1904.

THE CORRECTION OF THE STANDARDS OF WAVE-LENGTHS.

By J. HARTMANN.

In an earlier paper[1] I have pointed out the errors of Rowland's system of wave-lengths and their origin. The conclusions which I there drew have been meanwhile fully confirmed by Messrs. Perot and Fabry[2] and by Kayser.[3] The only difference of opinion which still exists refers to the manner which is to be adopted for creating reliable standards of wave-lengths. Although it is admitted both by Kayser and by Perot and Fabry that for all spectroscopic investigations the *absolute* value of the wave-lengths is a matter of entire indifference, they nevertheless come to the conclusion that simultaneously with the correction of the relative wave-lengths the transfer should be made from the system of Rowland to the absolute system of Michelson. Inasmuch as great confusion would be introduced in all statements as to wave-lengths by the very considerable change of the values of all wave-lengths involved in this operation, and inasmuch as the utilization of all measurements up to this time would be rendered much more difficult, I have proposed that this entirely useless change should not be made, and at the same time I suggested the very simple way of obtaining a new and wholly correct system of wave-lengths which fitted Rowland's as closely as possible. That feature of my proposal which is opposed by Kayser depends for the most part on an incorrect understanding of my article, and in view of the importance of the matter I regard it as desirable that I should once more clearly set forth the relations of things.

The demands to be made of a system of wave-lengths are wholly different according to the object for which they are to be employed. If a system of standard lines is principally to serve for furnishing the

[1] ASTROPHYSICAL JOURNAL, **18**, 167, 1903; *Zeitschrift für wissenschaftliche Photographie*, **1**, 215, 1903.

[2] ASTROPHYSICAL JOURNAL, **19**, 119, 1904; *Ann. de Chim.*, (8) **1**, 5, 1904.

[3] ASTROPHYSICAL JOURNAL, **19**, 157, 1904; *Zeitschrift für wissenschaftliche Photographie*, **2**, 49, 1904.

fundamental net for laboratory determinations of wave-lengths, from which all the other wave-lengths are to be determined by interpolation, then the principal issue is that the errors of this system of standards shall have a *regular* course, and that individual lines shall not show large random deviations. For if the latter were the case, as Kayser rightly points out, very different wave-lengths would result for the dependent lines according to the choice of a standard. Hence for the purpose named it would not be important to have a system of rigorously correct *relative* wave-lengths, but only that its errors should have such a continuous progression within long stretches, that the interpolation should at no point be rendered difficult.

A second requirement for perfectly correct *relative* wave-lengths is made on the system of standards in the case of all those applications where widely separated portions of the spectrum are brought into mathematical relations to each other. This is the case for interference methods, for observations of coincidences with gratings, and for all investigations as to the series structure of the lines.

The third requirement, that the *absolute* values of the wave-lengths should also be correct, arises only in those cases where the wave-length of light is to serve as the measure for other magnitudes. In all these purely metrological operations the case is, however, simplified by the fact that the observer always makes use of certain definite and entirely distinct kinds of rays, as the cadmium lines, the wave-lengths of which can be adopted according to what seems to be the most correct values obtained up to the time they are required.

If we keep the actual practical necessities before our eyes in this way, we shall obtain the following succession of operations which must be executed, arranged in order of their urgency:

The first and most urgent problem is the development of a system of *uniform* standards extending over the whole spectrum; that is, a system all the lines of which should be so fully assured by careful measurements on numerous good grating negatives that the accidental errors of the individual lines should not exceed the value of 0.003 tenth-meter.

In my previous essay I showed first how, by the application of empirical corrections as well as by the employment of plates of very different quality for the separate lines, Rowland introduced errors in

his standards in the solar spectrum and in his standards in the arc spectrum, which were distributed altogether irregularly and are now wholly beyond checking. I therefore expressly emphasized the fact that *for just this reason all of Rowland's standards are not to be used as a basis for accurate measurements.* I further showed that for our purpose there are now only two systems which can come into consideration, namely: first, the wave-lengths from the *Preliminary Table of Solar Spectrum Wave-Lengths* (which I designated by P. T.), and, secondly, Kayser's standards from the arc spectrum of iron, which I designated by K.

Thanks to the excellent plates of Rowland and the careful measures of them by Jewell, the values of the P. T. are to be regarded as a perfectly *uniform* system, satisfying the first of the requirements stated. But indispensable as the use of this system is for the astrophysicist—particularly in the measurement of stellar spectra of Types II and III—the use of the solar spectrum for a comparison spectrum for photographs taken in the laboratory commends itself very little to the physicist. Hence, with entire propriety, Kayser's iron spectrum has been generally adopted as the comparison spectrum for all determinations of wave-lengths of terrestrial sources of light. It is, indeed, dependent upon the *irregular* system of Rowland's standards in the arc spectrum of iron, but by his careful measurements of numerous grating plates Kayser has smoothed out as far as possible the irregularities of the standards, so that his standards from the arc spectrum of iron may also be regarded as a sufficiently uniform system. My proposals refer essentially to the use of these two *uniform* systems, whence may be recognized the lack of foundation of Kayser's objection[1] that the adoption of my suggestion would leave certain larger errors unchanged.

Kayser's standards unfortunately end at λ 4495, and therefore we must regard a uniform continuation of the iron standards through the whole visual spectrum as the most important and urgent problem. Hitherto the observer has always served his purpose by using Kayser's values for wave-lengths shorter than λ 4500, but for longer wave-lengths has simply adopted the values of the P. T. for the solar spectrum in p ace of the wave-lengths of the arc lines of iron. This

[1] *Zeitschrift für wissenschaftliche Photographie*, **2**, 53, 1904.

procedure is wholly unpermissible, as the positions of the lines in the arc spectrum deviate in an irregular manner from those in the solar spectrum. If it was desired to establish a direct connection with the values of the P. T., the solar spectrum should be photographed as a direct comparison spectrum, which presupposes an experienced observer and a very perfect adjustment of the spectrograph, as can now be attained in astrophysical apparatus.

The unavoidable coexistence, both present and for the future, of the *two* systems of standards now leads us to the problem of accurately determining their relation, which is of first importance for astrophysics. Designating by λ_P the wave-length of any given line measured precisely on the system of the P. T., and by λ_K the wave-length of the same line referred to the system of K., I placed in my former paper

$$\lambda_P = \lambda_K + k,$$

and I was able to give in Table X provisional values of the correction k for the region from λ 3400 to λ 4500.

The corrections k have their origin for the most part in the empirical correction applied by Rowland to his standards in the arc spectrum. It is therefore to be recommended that in a new working over of Kayser's standards the K. system should be so altered by the employment of the corrections k so that it is perfectly identical with the P. T. system. I shall revert to this point at the close of this article.

Although the solution of the problem thus far discussed, the creation of a *uniform* system of standards, is readily attainable with our present apparatus, and indeed may be regarded as almost completed, except for the necessary extension of Kayser's standards, the second problem, that of establishing a system of correct relative wave-lengths, encounters difficulties very great and, as it would appear from Kayser's paper, at present insurmountable. This indicates the necessity of beginning now to arrange all spectroscopic measures so that it shall be possible to easily transfer them to another rigorous system of relative wave-lengths, if it later becomes possible to establish such a system. For this purpose, according to what has been said above, nothing more is necessary than that all present measurements should be connected as closely as possible to one of the two above-mentioned uniform systems of standards by the use of a large number of comparison lines. Then, as soon as the system of the P. T. can be made

a rigorous relative system by the application of the corrections C, at present unknown, the same corrections will have to be given to all wave-lengths referred to the P. T.

I can therefore by no means agree with the rather unhappy opinion of Kayser that all previous determinations of wave-lengths, with the sole exception of Hasselberg's, are worthless if a precision of 0.01 tenth-meter is requisite, and that all determinations of wave-length are now useless pieces of work. I am, on the contrary, of the opinion that all measurements which are carried out with sufficient precision, that is, to 0.001, and are rigorously referred to one of the uniform systems above named, will retain their value permanently. The value of these determinations of wave-length is not at all affected by the question of when and with what accuracy the corrections k and C can ultimately be determined. The employment of these two uniform, but not rigorous relative systems of standards can, of course, be only a temporary expedient, and efforts will be made to replace them as soon as possible by a system of the most accurate possible *relative* wave-lengths. The situation is, however, quite different as to the question whether we should not on this occasion also leave the *absolute* system of Rowland and go over to that of Michelson. It is, indeed doubtless to be regarded on first consideration as the ideal arrangement to establish a system whose wave-lengths correspond with absolute precision to the standard meter. But this idea must be regarded as Utopian, as soon as it appears that it will never be fully attainable; indeed, that its attainment would be, not only rather useless, but also even associated with great disadvantages. This case, for instance, is before us in regard to the standards of wave-length. With all respect for the fundamental work of Michelson and Benoit, it would nevertheless be assumed by no one that it furnishes a value of the relation of wave-length to the meter valid for all time. Michelson himself gives a series of corrections which should be taken into account in a repetition of his measurements. Every such repetition of the fundamental measurements which, with the increasing precision of our methods of observation and apparatus, must lead to increasingly sharp values, would involve a change of all wave-length data, if it was desired constantly to work with correct absolute wave-lengths. This would be wholly incompatible with the unity which is absolutely

necessary for such work. It would therefore finally be compulsory, after a longer or shorter time, to adopt my suggestion, namely that we should stop at some one system of *relative* wave-lengths, so that for all later absolute measurements it would be practically necessary to redetermine only the reduction factor, which I have designated by F_0.

The conditions before us are precisely analogous to those in the case of the introduction of the metric system. Inasmuch as the meter is defined as the ten-millionth of the Earth's quadrant, it will be necessary to correspondingly correct the length of the meter after every geodetic measurement of an arc, if we wished to employ an absolutely correct metric measure. But since this is not consistent with the necessary constancy of all units of measure, the only correct escape has been selected, by the legal establishment of a new standard for all time, the idea of the *absolute* correctness of the measure being abandoned. An entirely similar step was taken in astronomy in the definition of the "astronomical unit," that is, in the transition from terrestrial to cosmical dimensions; and it is just this that I have proposed for the transition for the microcosmic system of measurements. This step will in any case be always unavoidable, if the relative measurements are decidedly more accurate, through any range of measurement, than the absolute observations which are made for establishing a connection with the next larger or smaller unit of measure.

If, therefore, according to what has been said, we shall be sooner or later compelled to stop at some system of wave-length which is relatively, but not absolutely, correct, it would be a wholly useless burdening of all spectroscopic work to change at this time from the system of Rowland to that of Michelson. My proposal may therefore be briefly stated as follows:

Let the wave-length of the red line of the cadmium spark in a vacuum measured in air at $+20°$ C. and 760 mm pressure be adopted as

$$\lambda = 6438.6911,$$

as the invariable fundamental value for all time. The measurements of Michelson, Hamy, Fabry, and Perot then will yield values for the lines of *Cd*, *Hg*, *Zn*, *Fe*, *Cu*, *Ag*, *Li*, and *Na*, which were given in

Table XI of my previous paper. These wave-lengths, obtained by the interference method, are without doubt very reliable, but, as I would here expressly point out, they do not at present constitute a *uniform* system, inasmuch as the mode of their origin does not preclude one or the other of these values from being erroneous by a considerable random amount. Therefore, on the one hand, the fundamental references of these lines to the red cadmium line should be repeated, while, on the other hand, their wave-lengths should be adjusted by measurements on numerous good grating plates, and these connections and adjustments should be extended over the whole of the rest of the spectrum. Every strictly relative system of standard lines derived in this way will differ only immaterially from that of Rowland, so that the corrections C to be applied to the earlier measures will always be very small, in many cases indeed negligible.

It is to be hoped that, in accordance with the above-mentioned *most urgent* need, the possessors of large grating spectrographs will first, in continuation of Kayser's iron standards, establish a system of iron lines as uniform as possible, and hence well adjusted, referred to the following standards from the arc spectrum of iron:

4309.533	4383.709	4466.723	5233.132
4315.246	4391.121	4469.552	5302.501
4325.931	4404.913	4476.193	5434.710
4337.207	4415.285	4484.406	5506.970
4336.726	4427.474	4489.915	5586.965
4352.897	4430.785	4494.741	5615.848
4358.675	4442.507	4736.946	5763.219
4367.744	4447.892	4859.928	6065.695
4369.939	4454.558	5002.057	6230.945
4376.089	4461.824	5083.518	6495.213

The first lines of this list, down to that at $\lambda 4495$, were obtained from Kayser's standards by applying the above-mentioned correction k according to Jewell's observations, and they therefore constitute a system as uniform as possible in rigorous agreement with the P. T. The corresponding figures for the preceding part of the spectrum from $\lambda 3400$ to $\lambda 4300$ are similarly obtained by applying to Kayser's wave-lengths in values of k given in Table X of my previous paper. The values from $\lambda 4737$ onward rest on the measures of Perot and

Fabry, and, if their measures are freed from error, constitute a rigorous relative system referred to the cadmium line λ6438.6911. It is also rigorously comparable with the P. T., if the latter are freed from their errors by the application of the corrections *C* given in my Table VII.

Astrophysical Observatory, Potsdam,
April 1904.

THE AFTERGLOW OF METALLIC VAPORS IN NITROGEN —A NEW BAND SPECTRUM.

By PERCIVAL LEWIS.

CERTAIN varieties of afterglow or phosphorescence in vacuum tubes have been studied by various investigators.[1] In most of these cases the phenomenon was observed either in mixtures of gases or in single gases which had not been purified with great care; the discharge employed was either the simple induction current, without spark-gap and capacity, or the electrodeless discharge, and in most cases the spectrum, when observed, was found to be continuous. In cases where it was described as discontinuous, details of its character are lacking, and no observations on the ultra-violet region seem to have been made.

In 1899 the writer observed an afterglow in nitrogen which seemed different in several particulars from those previously described.[2] It was found only in nitrogen as pure as could be readily obtained. It was produced only by a strong discharge with spark-gap and condenser in circuit, no trace of it being seen when the simple induction current was used. It appeared with almost equal facility at any pressure from a few millimeters to about 10 cm, and has since been obtained with electrodes close together at a pressure of 35 cm. The spectrum was discontinuous, consisting in the visible region of several strong diffuse lines or bands in the red, yellow, and green. The thin bright path of the discharge was surrounded by a chamois-yellow phosphorescent aureole, filling the entire tube at low pressures and extending 10 or 15 cm in both directions into the connecting tubes. It remained visible several seconds after the passage of the discharge. After each discharge the luminous fog was propagated slowly through the gas in both directions from the electrodes. As in a similar case observed by Warburg,[3] whenever fresh gas was admitted the phos-

[1] KAYSER, *Handbuch der Spectroscopie*, 1, 249; NEWALL, *Proc. Camb. Soc.*, 9, 295, 1897; GOLDSTEIN, *Verhand. d. deutschen Phys. Ges.*, 110, 1900.

[2] LEWIS, ASTROPHYSICAL JOURNAL, 12, 8, 1900; *Ann. der Phys.*, 2, 249, 1900.

[3] *Arch. des sc. phys. et nat.*, 12, 504, 1884.

phorescent aureole was driven like a luminous cloud before it. The aureole was instantly destroyed by the admission of very small traces of any foreign gas, such as hydrogen, carbon dioxide, water vapor, and especially oxygen. The nitrogen used was prepared by heating a solution of sodium nitrite and ammonium sulphate, and was carefully freed from oxygen by passing through pyrogallic acid, and from water vapor by passing it through a train of drying tubes. This peculiar form of afterglow could not be obtained from nitrogen prepared from air.

Recently the writer has made further spectroscopic observations of this afterglow, which were extended into the ultra-violet by the use of a quartz spectrograph. The gas was prepared as before, and passed through an alkaline solution of pyrogallic acid, a concentrated solution of KOH, and tubes containing solid KOH, soda-lime, and phosphorus pentoxide. It seems likely that traces of NO are present in nitrogen prepared in this manner.[1]

SPECTRUM OF THE AUREOLE.

Photographs were first taken of the spectrum of the aureole while the discharge was passing. As the aureole extended several centimeters from the electrodes, the use of a bent tube enabled the radiation from a long column of the aureole to fall on the slit of the spectrograph, while it was completely screened from the light of the direct discharge. On account of comparatively feeble luminosity, exposures lasted an hour or more.

The spectra thus photographed were discontinuous, containing many lines and bands in the ultra-violet, besides the lines previously observed in the visible spectrum. The remarkable fact was brought out that a number of metallic lines were present. All the stronger mercury lines, due to vapor diffusing from the pump, were found, the strongest line in the entire spectrum being the mercury line at λ 2537. When aluminum electrodes were used, the stronger aluminum lines were usually, but not always, found. The conditions determining their appearance have not been determined, but it seems likely that they do not appear when very small traces of oxygen or water vapor are present. The stronger nitrogen bands of Deslandres' second and third groups appear on the plates, but none of the first group

[1] KREUSLER, *Ann. der Phys.*, **6**, 419, 1901.

could be detected visually with a pocket spectroscope of great power. There were also a number of other lines and bands which have not yet been identified.

With platinum electrodes the aureole was unchanged in general appearance, and the visible spectrum was the same as before, but the ultra-violet region was somewhat different. The mercury lines were strong. The nitrogen bands were relatively weak. The platinum lines at λλ 2629, 2655, 2660, 2706, 2734, 2830, 2930, 2998, 3060, 3157, and 3408 were apparently present. Their coincidence with lines of the spark between platinum terminals in air seemed perfect. Several new bands with edges toward the violet were especially prominent.

With iron electrodes the same band spectrum was given as with platinum, but no metallic lines, except those due to mercury, could be found—except possibly the strong iron lines at λ 2788 and λ 4325.

With zinc electrodes the strongest zinc lines were found—λλ 4811, 4722, 4680, 3345, 3303, 3282, 3072, 3035, 3018, 2801, 2771, 2756.

Some of the results obtained are shown in Plate IV. *A* is the spectrum of the direct discharge, without condenser, showing the bands of the positive column and the negative bands; *B* is the spectrum with condenser in circuit, showing both lines and bands. The exposure lasted about five minutes. *C* is the spectrum of the aureole at a pressure of 5 mm, exposure nearly one hour. The aluminum and the mercury lines are strong, while the nitrogen bands of the second group are weak. *D* is the same, taken at a different time with another tube at a pressure of 15 mm. The nitrogen bands are stronger, and several new lines and bands appear. The differences in relative intensity of *C* and *D* have not been accounted for. It may be due to changed electrical conditions. Two very strong and characteristic lines in the yellow and green are seen to have no correspondence with the nitrogen line or band spectrum, as shown in *B* and *A*.

Eye observations were made of these lines, and of another in the red which does not appear on the plates, with a spectroscope of great dispersion, and more exact determinations of wave-length were made than those previously published. There was a strong diffuse line at λ 6245, with a faint line on each side, of wave-lengths λ 6320 and λ 6175. The yellow line was separated into three pairs, of wave-lengths 5805–

45, 5800–5780, and 5760–35. The central pair was the strongest. These lines lie very near the mercury pair λ 5790–69, but comparison with the usually much stronger, but in this case barely perceptible, green mercury line makes it doubtful whether that pair would be visible under these conditions. The green line was found to be a band of fine lines, with head toward the red at λ 5408. A fainter band with sharp edge toward the violet was seen at λ 5005. All these lines and bands lie near lines or bands of the known nitrogen spectrum, but their general appearance and relative intensities indicate a different origin. The red, yellow, and green lines are the most characteristic and uniform of the new lines found in the aureole.

J is the spectrum of the aureole with platinum electrodes; *K*, the same with iron electrodes. *L* is the spectrum of the aureole in a commercial tube filled with NO_2, which also shows an afterglow (produced by either the simple induction or the condenser discharge). The visible spectrum, even when examined with high dispersion, appeared perfectly continuous, with a strong maximum in the red and yellow; in the ultra-violet the second group of nitrogen bands appears. The glass walls prevented observation any further into the ultra-violet. The spectrum of the direct discharge appeared to be identical with that of nitrogen.

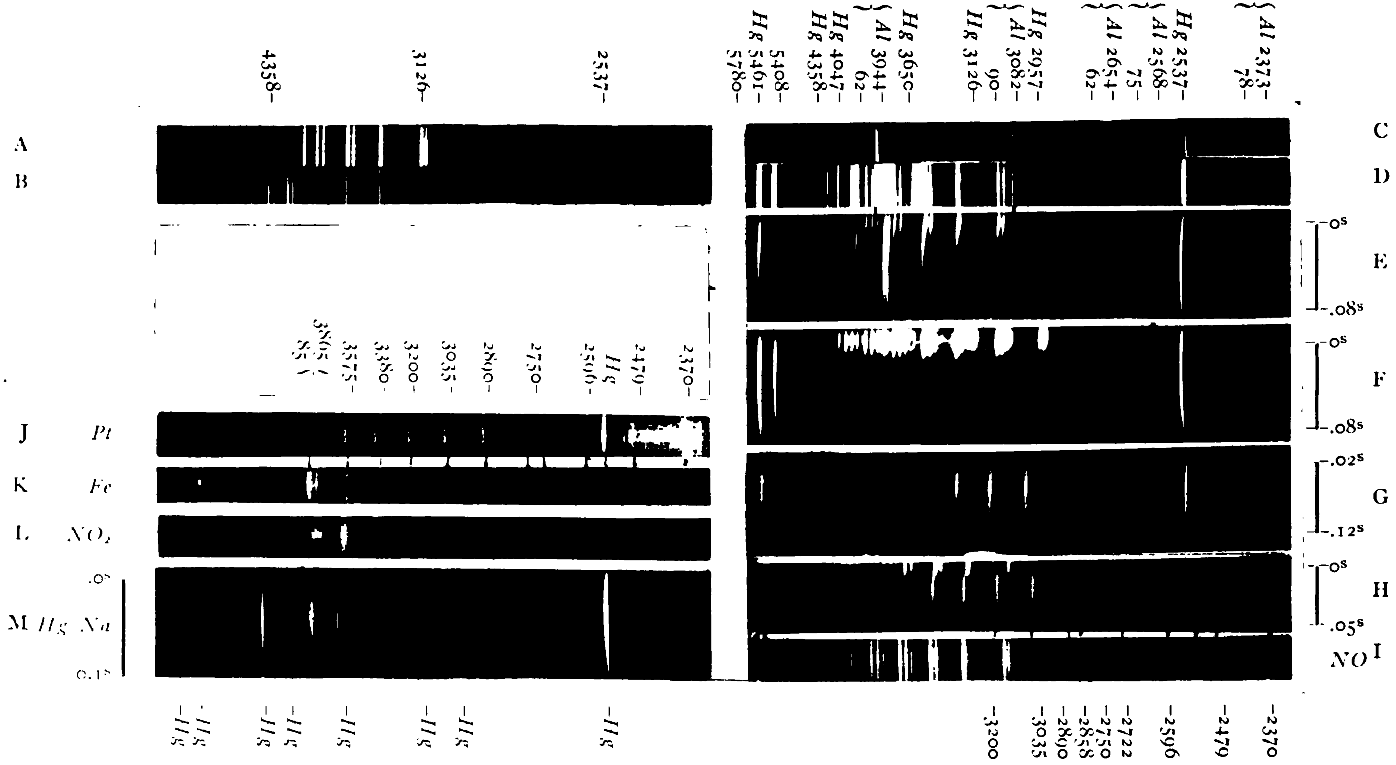
Photographs taken in connection with a previous investigation,[1] using a large glass prism and photographing simultaneously the spectrum of each part of the tube, showed that the aluminum lines at λ 3944 and λ 3962 were uniformly strong throughout the aureole, up to a distance of about 5 cm from the electrodes; but when there was no aureole showing the afterglow these lines could be found only in the immediate vicinity of the electrodes. Without the condenser and spark-gap, the aluminum lines appeared only at the negative electrode. This indicates that aluminum vapor or electrons must have diffused throughout the phosphorescing mass of gas, taking part in the processes giving rise to the afterglow.

SPECTRUM OF THE AFTERGLOW.

Observations were next made on the spectrum of the afterglow which persisted when the current was interrupted. In front of the spectrograph slit was placed a large metallic disk, provided with

[1] LEWIS, ASTROPHYSICAL JOURNAL, 17, 263, 1903.

PLATE IV.



SPECTRA OF THE AUREOLE AND AFTERGLOW IN NITROGEN.

ten projecting sectors (like Fizeau's toothed wheel), with intervals of several centimeters between them, which acted as intermittent screens when the disk was slowly rotated at a uniform rate by an electric motor. An adjustable contact allowed the current to be broken at any given phase of the sectors. These traveled downward in front of the slit, exposing the top first and the bottom about 0.1 second later. There was an integral exposure of each part of the slit about 0.3 second in duration, and then the next sector crossed the field, the circuit was closed, and the process was repeated. Some very interesting results were obtained, as shown in Plate IV, which is described below:

E. Pressure 15 mm; exposure, 2 hours. The current was broken when about one-fifth of the slit was exposed, so that the top shows the spectrum of the discharge. When the sector was at 0^s the current was interrupted, and beyond this point the spectrum of the afterglow proper appears, the lower part of the slit not being exposed until about 0.08 sec. after the discharge. The mercury line and a number of the characteristic afterglow lines and bands appear with almost undiminished intensity throughout this interval. The aluminum pairs at 3962–44 and 3093–82 appear in the spectrum of the afterglow, and persist fully 0.06 sec. In another plate they were found up to the very limit of 0.08 sec. Before each exposure the spectrograph was removed, the sectors were adjusted, and eye observations were made to be certain that no light from the direct discharge could enter below the assigned points on the slit. Even when the sectors were set so that no light at all from the discharge could pass beyond the wheel, a pocket spectroscope showed the characteristic visible afterglow lines and alongside of the band at λ 5408 was always seen the green mercury line at λ 5461, as shown in *D* and *E*.

F. Pressure about 4 mm; otherwise the same as above, but with slightly wider slit. The mercury line at λ 2537 seems as strong as before, but the other mercury lines are very weak. The aluminum lines do not appear. Some of the characteristic bands are stronger than at the higher pressure, some weaker. The stronger nitrogen bands of Deslandres' third group appear, and are persistent in the afterglow, although the bands of the second group disappear in this and the preceding case almost simultaneously with the break in the current.

G. Pressure 20 mm; exposure, 2 hours. The sectors were set so that exposure did not occur until about 0.02 sec. after the discharge ceased, the lower end of the slit being exposed 0.1 sec. later. This appears to be a continuation of the effect shown in *F* rather than of *E*, as might be expected from the pressure. Many such irregularities of relative intensity, and other characteristics which have not yet been accounted for, occurred frequently.

H. This is similar to *F*, except that the exposure is longer. This is of special interest because of the absence of the mercury line at λ 2537. This was due to the formation of a moist plug in a phosphorus pentoxide drying tube, which prevented the diffusion of mercury vapor from the pump. This is additional evidence that the mercury vapor plays no essential part in the origin of the strange bands and lines.

I. The spectrum of the discharge through NO, giving the bands of the second and third group for comparison with the above. This tube gave no afterglow with any kind of discharge, although a commercial NO tube gave the same afterglow as the commercial NO_2 tube (*L*).

The following metallic lines were found in the spectrum of the aureole, the strongest occurring also in the afterglow: mercury—$\lambda\lambda$ 5790, 5769, 5461, 4358, 4047, 3650, 3126, 2957, and 2537; aluminum—$\lambda\lambda$ 3962, 3944, 3093, 3082, 2662, 2654, 2575, 2568, 2378, 2373.

The lines of zinc and platinum found in the spectrum of the aureole have already been given. Those of zinc do not appear in the spectrum of the afterglow, and as yet the spectrum of the afterglow with platinum electrodes has not been photographed.

E. Wiedemann and Schmidt[1] have shown that metallic vapors will fluoresce under the action of light, but this seems to be the first case on record where phosphorescent radiation has been obtained from metallic vapors a measurable time after the electrical or luminous stimulus has ceased. Crew[2] found that the radiation of metallic vapors in the electric arc ceased within 0.001 sec. after the stopping of the arc. Whether the effect is of chemical origin, due to combinations between the metals and the gas, or whether it is caused by a readjustment of electrical equilibrium following the disturbance

[1] Astrophysical Journal, **3**, 207, 1896. [2] *Proc. Am. Acad.*, **33**, 337, 1898.

caused by the current, remains to be discovered. Small traces of oxygen are fatal to the afterglow, perhaps on account of the avidity with which this gas combines with any metallic vapors or free gaseous ions which may be present.

A NEW BAND SPECTRUM.

On most of the plates showing the afterglow spectrum there are a number of bands, some with edges toward the violet, which have not yet been identified. They are different in position and appearance from any of the known nitrogen bands. They appear when no mercury vapor is present, and cannot be due to it. They are likewise found when no phosphorus pentoxide drying tubes were used, so that they are not due to vapors from that. Although every effort was made to avoid contamination, it is practically impossible to avoid impurities from the materials used to generate the gas, from the drying tubes, and from the grease on the stop-cocks (a mixture of vaseline and paraffin), and the lines may arise in this way.

Comparison with *I* (spectrum of NO) may give a clue to the origin of some of these bands. In *F*, *G*, and *H*, several bands of the afterglow are evidently coincident with the strong bands of the third group in *I*. Still stronger bands of the afterglow in *F*, *G*, *H*, and *J*, of approximate wave-lengths 2750, 2890, 3035, and 3200, with edges toward the violet, apparently coincide with very weak bands in *I*. Other bands of wave-lengths 3380, 3575, and 3805 cannot be found on *I*, perhaps because strong bands of the second group overlie them. Some of these bands are almost coincident with bands of the second group, but appear to be different. Other bands at λ 4130 and λ 4540 are found on *D*, and *E* but not on the other plates. It seems possible that some of these bands are due to the presence of NO. None of them are to be seen in the spectrum of NO_2 (*L*), but they may be due to other oxides of nitrogen, or to compounds of nitrogen with impurities.

Especially prominent on *D*, *E*, and *M*, in the afterglow, are two bands of wave-lengths near 3865 and 3885. They much resemble the cyanogen bands found in the spectrum of the Bunsen flame,[1] and may possibly be due to that compound. The wave-lengths of these bands are given by Eder as 3873 and 3890.

[1] EDER, *Wiener Denkschriften*, **57**, 551, 1890.

No special attempt was made at accuracy in these determinations of wave-length, which may be in error by 5 units or more. Further measurements will be postponed until a spectrograph of much greater dispersion is available.

It was found in previous investigations that the afterglow was destroyed by heating metallic sodium in the tube, presumably thus reducing any oxides. It seems, however, that this effect was probably due to impurities in the sodium; for after heating it repeatedly to drive off volatile ingredients, it was found that the afterglow could be obtained although not quite so strong as before. It is a remarkable fact, however, that the sodium lines did not appear in the spectrum of the aureole or of the afterglow.

Mercury-sodium amalgam was heated in the tube. In this case the afterglow appeared in full strength, but when the tube was heated the mercury vapor seemed to be dominant, giving a greenish-white color to the afterglow. It was a real afterglow of the mercury vapor, persisting more than 0.1 sec., as shown by *M*. The mercury lines are very much enhanced, the bands of the afterglow somewhat weakened. It must be repeated that numerous efforts failed to find any afterglow of any metallic vapor unless the conditions were such as to cause an afterglow in the gas.

When the mercury-sodium amalgam was used, the sodium lines were also absent, even when the tube was highly heated. This is singular, considering that sodium is so much more volatile than aluminum.

Goldstein[1] has described some forms of afterglow which may be similar to that here described. He also refers to some unpublished observations of Hertz on the spectrum of an afterglow in nitrogen giving a discontinuous spectrum, but precise details of such spectra have never been published, and no one has apparently observed the very significant fact that metallic vapors may take part in the phenomenon.

The writer will further investigate the electrical conditions accompanying the afterglow, and endeavor to find the origin of the new bands described. It will also be of interest to find whether such

[1] *Verhand. d. deutsch. phys. Ges.*, p. 110, 1900.

afterglows of metallic vapors may take place in other atmospheres than nitrogen.

The results so far obtained may be summarized as follows:

1. The afterglow described occurs only in nitrogen chemically prepared, dried and purified as far as possible, but probably containing traces of *NO*.

2. This afterglow can be produced only by a strong condenser discharge.

3. Its spectrum is discontinuous, consisting of lines and bands.

4. Some bands are those of Deslandres' third group, others are of unknown origin. Some of the lines are due to mercury and aluminum vapor, others are unidentified.

5. The mercury and aluminum vapors phosphoresce at least 0.1 sec. after the discharge has ceased. There is no afterglow of the metallic vapors unless there is an afterglow in the gas.

6. The mercury lines appear equally at all pressures; the aluminum lines are usually stronger at relatively high pressures—2 cm or more—but are sometimes altogether absent.

7. With heated mercury-sodium amalgam in the tube, the afterglow still persisted, but seemed largely localized in the mercury vapor, the intensity of the other afterglow lines, being less, and the afterglow turning white.

Mr. P. E. Rowell rendered very valuable assistance during this investigation.

University of California,
Berkeley, May 24, 1904.

NOTES ON THE SPECTRA OF NITROGEN AND ITS OXIDES.

By PERCIVAL LEWIS.

THE SPECTRUM OF THE ELECTRODELESS DISCHARGE.

SOME general observations on the spectrum of the electrodeless discharge in various gases and in gaseous mixtures showing an afterglow have been made by J. J. Thomson[1] and by Newall,[2] but no systematic work has been done on such spectra. If it be assumed that the ring discharge is due solely to electromagnetic induction, there can be no passage of electricity between electrodes and the gas, and no finite discontinuities, nor even a potential gradient in the path of the current. This, and the fact that the pressure in the gas is much less than that ordinarily employed in vacuum tubes, render it possible that some characteristic peculiarities may be observed in such spectra.

Nitrogen is a particularly interesting subject to study in this connection, on account of the multiplicity of its spectra, and because it has several groups of bands which behave quite differently under different physical conditions. As it is often assumed that the spectrum of the negative pole is due to cathode rays, it is also of interest to see whether these bands appear when there is no cathode.

Photographs were taken with a quartz spectrograph. The discharge tube was a spherical glass bulb, about four inches in diameter and provided with a quartz window. Around it was a pasteboard cylinder on which were wound eight turns of heavy wire in circuit with spark-gap and condenser. The pasteboard cylinder was moistened to screen the tube from electrostatic effects, and the helix connected to earth. The ring was always clearly defined.

The nitrogen used was prepared by heating a solution of sodium nitrite and ammonium sulphate.[3] It was freed from traces of oxygen by bubbling through an alkaline solution of pyrogallic acid, and from

[1] *Phil. Mag.*, **32**, 1892. [2] *Proc. Camb. Soc.*, **9**, 295, 1897.

[3] Nitrogen prepared in this manner contains traces of NO. (See KREUSSLER, *Ann. der Phys.*, **6**, 419, 1901.)

carbon dioxide and water vapor by passing it through a concentrated solution of caustic potash and several drying tubes.

The discharge appeared at pressures of about 0.1 mm. At first it was of coppery-pink color, characteristic of pure nitrogen. At somewhat lower pressures it turned white, as likewise observed by J. J. Thomson; but the spectroscope showed that this whiteness was due to mercury vapor from the pump, not to any change in the nitrogen, nor even to carbon compounds, the spectrum of which was vanishingly weak. Photographs were usually taken at pressures of about 0.05 mm.

Deslandres[1] showed that in the spectrum of the positive column in nitrogen there are three distinct groups of bands, which are differently affected by physical conditions. The first group comprises most of the visible spectrum, and is attributed by him to pure nitrogen. The second, which begins in the violet and extends to about λ 2800, is supposed by him to be due to nitrogen and hydrogen. The third begins about λ 3000 and extends to about λ 2100. This group is considered by Deslandres to be due to traces of oxygen, forming an oxide, as metallic sodium heated in the tube will eliminate it.

In the photographed spectrum of the ring discharge, shown in *A*, Plate V, the third group is entirely lacking. The second group is strong. The first group, as observed visually, was fairly strong. The difference between this spectrum and that of the simple induction discharge with electrodes is shown by comparison with *B* (spectrum of the negative pole) and *C* (spectrum of the positive column), taken at a pressure of about 5 mm. The differences may depend on differences of pressure as well as of electrical discharge. The stronger mercury lines are all present, as well as several other sharp lines which have not been identified.

It is interesting to note that the negative bands appear on the plates, as may be seen by comparison with *B*. The only apparent way to reconcile this with the view that the negative bands are due to cathode rays seems to be to assume that there are electrostatic discharges from the glass walls, which thus act as electrodes. This is in accordance with some other results obtained by the writer.[2] Hardèn[3] and

[1] *Ann. Chim. et Phys.*, **15**, 46, 1888.

[2] ASTROPHYSICAL JOURNAL, **17**, 266, 1903.

[3] *Physikalische Zeitschrift*, **5**, 74, 1904.

Lecher[1] have recently shown that the ring discharge as ordinarily produced is largely due to electrostatic discharges from the walls of the tube.

Some believe that the spectrum of the negative pole is due to the higher temperature at the cathode. It was found that the negative glow could easily be produced at any point of the bulb by simply placing the finger at that point , and no very high temperature seems to be possible under such conditions; nor is the ring discharge sufficiently violent to allow us to explain the appearance of the negative bands as a result of the intensity, as suggested by Goldstein.[2] It seems highly probable that the negative bands are not primarily due either to high temperature or to great intensity of discharge, although some recent work of Hemsalech[3] shows that the negative glow may be greatly intensified by heating the electrode.

THE THIRD GROUP OF BANDS.

The third group of bands, with principal maxima at λλ 2370, 2479, 2596, 2722, and 2858, appear in the spectrum of the negative pole (*B*) and of the positive column (*C*), at pressures of 5 mm. They also appear in the spectrum of the simple spark between platinum terminals in nitrogen at 1 atmosphere (*H*) and at 2 atmospheres pressure (*I*). They do not appear when a small capacity is placed in parallel with the tube (*D*). If these bands be due to an oxide of nitrogen, as Deslandres suggests, it might be expected that the more intense discharge with the capacity would permanently dissociate this oxide and cause its spectrum to disappear.

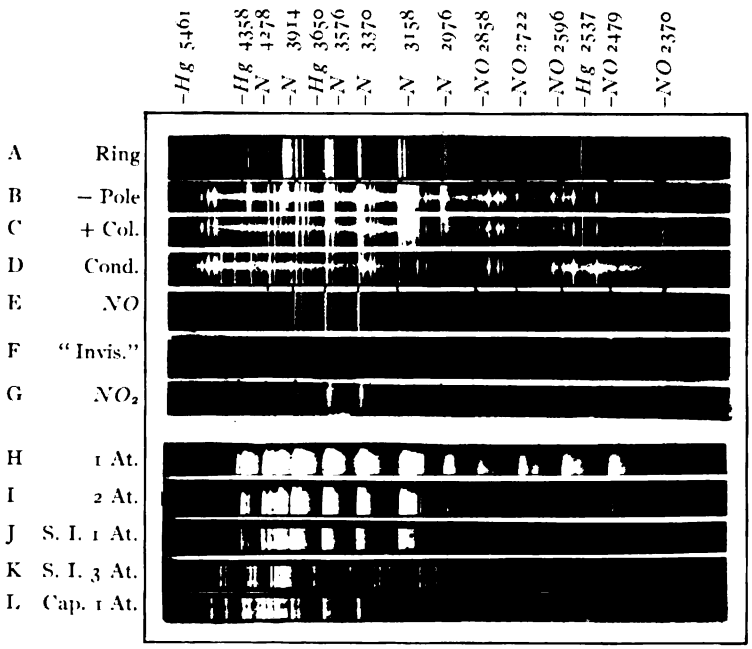
(A zinc comparison spectrum is superimposed on *B*, *C*, and *D*.)

E gives the spectrum of a tube filled with pure dry *NO*. It is much like *C*, but the bands of the third group are relatively much stronger. The same spectrum was obtained with external electrodes. giving a very feeble current, with inner exectrodes and a simple induction current, and with a small capacity in circuit. It was observed, however, that the tube fluoresced brilliantly during the first moments of the discharge; but this fluorescence gradually died out, and more rapidly as the intensity of the discharge was increased—

[1] *Ibid*, 5, 179, 1904. [2] *Wied. Ann.*, 15, 280, 1882.

[3] *Mem. Manchester Phil. Soc.*, 48, No. 10, 1904.

PLATE V.



–Hg 5461
–Hg 4358
–N 4278
–N 3914
–Hg 3650
–N 3576
–N 3370
–N 3158
–N 2976
–NO 2858
–NO 2722
–NO 2596
–Hg 2537
–NO 2479
–NO 2370
A Ring
B – Pole
C + Col.
D Cond.
E NO
F "Invis."
G NO_2
H 1 At.
I 2 At.
J S. I. 1 At.
K S. I. 3 At.
L Cap. 1 At.

presumably the effect of the gradual dissociation of the oxide, resulting in a diminished ultra-violet radiation. This is in accordance with results previously obtained by the writer,[1] which showed that a brilliant fluorescence of the tube accompanied the passage of a constant stream of fresh gas through the tube, and that this fluorescence was due to the ultra-violet radiation of the third group. These bands are attributed to *NO* on *B*, *C*, *E*, and *F*, as the evidence seems to justify Deslandres' view. As oxides likewise exist in nitrogen prepared from air, it seems probable that no one has ever studied the spectrum of pure nitrogen.

THE INVISIBLE DISCHARGE.

In the previous investigation, alluded to above, it was found that often when the pressure was too high for the discharge to pass, the glass fluoresced brightly around the electrodes. It has since been found that very brilliant effects may be obtained in this way at pressures of from 10 to 20 cm, when there was absolutely no visible discharge except an occasional feeble brush from the electrodes. Sometimes the tube fluoresced over the entire distance between the electrodes. The spectrum of this invisible discharge is shown in *F*. The pressure was about 15 mm, and the exposure was 30 minutes. The third group of bands is very strong, and the edges of the second group very distinct. It may be imagined that with such relatively feeble electrical disturbances the nitric oxide might be excited to radiation without dissociation. These results may have some connection with those recently obtained by Sanford.[2] He found that the metal of condenser plates or between condenser plates emitted invisible radiations with a maximum lying near $\lambda\lambda$ 3500–3700. This is the region of the strongest bands of the second group.

SPECTRUM OF NO_2.

The spectrum of the discharge in this compound seemed to be identical with that of nitrogen, so far as its radiation was transmitted through a commercial glass tube. The spectrum of the phosphorescent aureole showing the afterglow is given in *G*. This spectrum, even when examined with high dispersion, seemed perfectly continu-

[1] ASTROPHYSICAL JOURNAL, **12**, 8, 1900; *Ann. der Phys.*, **2**, 40, 1900.

[2] *Physical Review*, **18**, 366, 1904.

ous in the visible region, with a strong maximum in the red and yellow. In the ultra-violet it seems to consist solely of the bands of the second group.

THE SPECTRA OF NITROGEN AT HIGH PRESSURES.

H, *I*, *J*, *K*, and *L* are reproductions of some photographs taken by Mr. Joel Stebbins in this laboratory while he was a fellow of Lick Observatory.

H is the spectrum of the simple spark between platinum electrodes in nitrogen at 1 atmosphere pressure; *I* the same at a pressure of 2 atmospheres; exposure, 30 minutes. Little or no difference can be observed, except that the entire spectrum is weaker at the higher pressure, and that a few of the stronger platinum lines appear.

J is the spectrum of the spark with a capacity and a self-induction of several hundred turns in the circuit, as a pressure of 1 atmosphere; exposure, 6 minutes. It is much like *I*, except that a number of platinum lines appear.

K is the same as above at a pressure of 3 atmospheres. The character of the spectrum is entirely altered. It consists mostly of lines, with a few of the stronger bands of the second group.

L is the spectrum at 1 atmosphere, with capacity, but no self-induction; exposure, 5 minutes. This likewise consists of lines with a few bands, but some lines appear which are not seen on *K*, and conversely, the new lines being apparently air lines.

The lines which are enhanced on *K* appear to be the strong platinum lines $\lambda\lambda$ 2630, 2655, 2660, 2706, 2734, 2794, 2830, 2988, 3043, 3064, 3157, 3204, 3283, 3468, 3485, 3628, and others.

Comparison of *J*, *K*, and *L* shows that increased pressure with constant self-induction has the same effect as increased self-induction at constant pressure—that is, the metallic lines are enhanced and the air spectrum weakened. A further investigation of this effect is in progress in this laboratory.

UNIVERSITY OF CALIFORNIA,
Berkeley, May 22, 1904.

COMET 1903 BORRELLY AND LIGHT-PRESSURE.

By S. A. MITCHELL.

IN view of the great interest at present shown in the pressure of light and its theories, it would probably be of some consequence to see what bearing the comet of 1903, Borrelly, has on the question. That a ray of light exerts a pressure on any surface on which it impinges comes as a direct result of the Electromagnetic Theory of Light published in 1873 by Clerk Maxwell,[1] and, as was shown by Bartoli[2] in 1876, can be deduced from the second law of thermodynamics. The pressure of sunlight at the surface of the Earth Maxwell computed from the known constants of solar radiation to be 0.592×10^{-10} grams per sq. cm. (This value is changed somewhat by using the latest determination of the solar constant as derived by Langley and Abbot at the Smithsonian Institution.) At the Sun's surface, as was pointed out by Arrhenius,[3] the radiation is much more powerful, and amounts to 2.75×10^{-3} grams per sq. cm. If, then, we imagine at the Sun's surface a cubical block of water of 1 cm edge, the pressure of light on it would be one ten-thousandth part of its weight, since superficial gravity at the Sun is 27.47 times what it is on the Earth's surface. If we decrease the size of the water particle, the pressure diminishes as the square of the edge and the weight as the cube. Consequently, for a little cube of water with an edge 10^{-4} cm, *i. e.*, one micron, or μ, the pressure of the Sun's light on it is exactly equal to its weight, and for a smaller cube of water the pressure would be greater than the weight, and the particle would act as if gravity had become negative. If the drop of water were spherical in shape instead of cubical, the critical diameter, at which the repulsion due to light-pressure would be equal to the attraction of gravity would be $1.5\ \mu$. For other substances the diameter is inversely proportional to the specific gravity.

The vapors of comets, as we are informed by the spectroscope,

[1] *Electricity and Magnetism*, § 792. [2] *Il Nuovo Cimento*, **15**, 195, 1883.

[3] *Physikalische Zeitschrift*, **2**, 81, 1900; also, *Lehrbuch der kosmischen Physik*, 150, 1903.

appear to consist largely of hydrocarbons with a specific gravity about 0.8, and for these the critical diameter would be about 1.9 μ. If the vaporized portions of the comet form drops whose diameters are greater than this value, a tail will be formed pointing toward the Sun; but if the diameters are less than 1.9 μ, the tail will point away from the Sun. If it should happen that drops of different sizes are formed—and there seems to be no reason why this should not be possible—the comet will have several tails, as in the comet of 1744, which had five.

Such, in brief, is the theory of Arrhenius,[1] which explains away readily enough a great many of the difficulties connected with comets and their tails.

Since the light-pressure depends directly on the intensity of the Sun's radiation, which decreases inversely as the square of the distance, as is also the case with gravity, the ratio of pressure to weight is therefore a constant independent of the distance from the Sun. The manner in which this ratio is found was first shown by Bessel[2] in 1836, who computed the magnitude of the repulsive force from the curvature of the tail of the comet of 1811. Bredichin[3] more recently, from measures of many comets' tails, has found them to be of four different types, in which the repulsive forces are respectively 18.5, 3.2, 2.0, and 1.5 times the attraction of gravity; the straight tail, according to his ideas, consisting of hydrogen, the plume-like tail of hydrocarbons, and the short stubby one of metallic vapors, chief among which are iron and sodium. The electrical force, on which Bredichin explains his repulsions, has been shown by Lebedew[4] not to have a sound physical basis.

This objection cannot be raised to the principle of Arrhenius. That light exerts a pressure has been shown by Lebedew,[5] and Nichols and Hull;[6] the latter, indeed, have succeeded[7] in producing

[1] See the interesting articles by Cox, *Popular Science Monthly*, **60**, 26 , 1902.

[2] *A. N.*, **13**, 185, 1836.

[3] *Annales de l'Observatoire de Moscou*, (2) **1**, 45, 1886.

[4] Astrophysical Journal, **16**, 155, 1902.

[5] *Annalen der Physik*, (4) **6**, 433, 1901; Astrophysical Journal, **15**, 60, 1902.

[6] *Physical Review*, **13**, 307, 1901; Astrophysical Journal, **15**, 62, 1902.

[7] Astrophysical Journal, **17**, 352, 1903.

a laboratory comet's tail, although, as pointed out by them, other forces than light-pressure probably helped to give the repulsion.

However, a rigid application of the theory of Maxwell is possible only when the body acted upon is large compared with the vibrations of light itself. When the body is of a size approximating the wave-length of light, Schwarzschild[1] has shown that the maximum value of the repulsive force is about twenty times the attraction of gravity.

The measures by Sebastian Albrecht[2] of photographs of comet 1903, Borrelly, give the data for determining the curvature of the comet's tail and the magnitude of the repulsive forces forming the tail.

Using the parabolic elements by Perrine in *Lick Observatory Bulletin* No. 47, it is possible to compute the velocity of the comet along the radius vector and perpendicular thereto. Added to these two, there is a third velocity caused by light-pressure along the radius vector, but directed away from the Sun, and the resultant of these three gives the motion in the comet's tail. Combining together the motions to and from the Sun along the radius vector, the direction of the tail comes as the resultant of two velocities. As is easily seen, the tail must lag behind the radius vector. Conversely, knowing the velocity of the comet perpendicular to the radius vector, and the angle the tail makes with this line to the Sun, it is possible to calculate the velocities caused by light-pressure.

As it is impossible for the comet's tail to be ahead of the radius vector, or, using Albrecht's notation, for the angle between the radius vector and the tail to have a negative sign, such angles were neglected in the calculation for finding the values of the repulsive forces which follow:

[1] *Sitzungsberichte der math.-phys. Classe der k. b. Akademie der Wissenschaften zu München*, **31**, 293, 1901.

[2] *Lick Observatory Bulletin* No. 52; Astrophysical Journal, **19**, 121, 1904.

RATIOS BETWEEN REPULSION FORMING TAIL AND GRAVITY.

Plate No.	Date, 1903	P. S. T.	Principal Tail: Angle between Radius Vector and Tail	Principal Tail: Value of Repulsive Force	Secondary Tail: Angle between Radius Vector and Tail	Secondary Tail: Value of Repulsive Force
1	June 22	$14^h\ 42^m$	$-1^\circ.5$	......	$+15^\circ$	1.9300
2	June 23	13 37	+ 1.8	16.804	+15	1.9571
3	June 26	13 30	− 2.3	......	+12	2.6549
4	June 29	12 46	+ 2.5	12.674	+16	1.8802
5	June 30	13 33	+ 1.2	26.649	+17	1.8845
6	July 1	13 48	+ 4.4	7.318	+18	1.7751
7	July 12	8 52	+ 2.1	17.093	+20	1.6853
8	July 13	9 12	+ 4.5	8.057	+19·	1.8210
18	July 20	10 58	+ 0.4	(99.598)	....	......
19	July 20	14 40	− 0.4	......	....	......
20	July 23	11 45	+ 1.5	27.820	+16	2.5240
21	July 24	11 45	+ 3.9	10.864	+18	2.2794
22	July 25	12 00	+ 3.5	12.322	+22	1.8560
23	July 26	11 48	+ 0.6	(73.250)	+25	1.6755
24	July 27	10 20	+12.5	(3.505)	+25	1.6514
25	July 28	11 48	+ 4.2	10.852	+24	1.7983
26	July 29	11 53	+ 5.9	7.871	+26	1.6822
27	Aug. 11	8 49	+ 0.6	(114.940)	+35	1.7255
28	Aug. 12	9 00	+ 1.2	(58.925)	+36	1.6926
29	Aug. 13	9 12	+ 2.0	35.580	+44	1.3980
30	Aug. 14	9 05	+ 2.2	35.042	+43	1.4385
31	Aug. 15	8 55	+ 2.9	29.634	+43	1.6325
32	Aug. 18	8 36	+ 1.4[1]	(77.973)	+54	1.3694
			Mean	18.47	Mean	1.824

In taking the average for Tail 1, the quantities coming from angles less than 1°, or from angles widely discordant, as in Plate 24 were not included. Thus, neglecting the values in ( ), the mean value of the repulsive force is 18.47 times gravity. Naturally the values of the repulsive force for Tail 2 agree much better among themselves, and their mean is 1.824.

The last four values in the table, with a mean of 1.460, seem to show the existence of a third tail, and this is corroborated from the photographs of August 12 and 15.

The angles between the radius vector and the tail were computed from the mean values of the repulsive forces above determined, and results are given in the following table, where *O* represents the values

[1] By mistake this angle was printed $+14^\circ.1$ in Mr. Albrecht's measures. The value of the repulsive force was not included in the mean.

as measured by Albrecht, and C those computed. Values are also given for a third tail, for the last four measures only.

ANGLES BETWEEN TAILS AND RADII VECTORES.

Plate	Date, 1903	Principal Tail			Second Tail			Third Tail		
		O_1	C_1	O_1-C_1	O_2	C_2	O_2-C_2	O_3	C_3	O_3-C_3
1	June 22........	− 1.5	+1.6	− 3.1	+15.2	+16.0	−0.8	.....	.....	.....
2	June 23........	+ 1.8	+1.6	+ 0.2	+15.1	+16.1	−1.0	.....	.....	.....
3	June 26........	− 2.3	+1.7	− 4.0	+11.5	+16.5	−5.0	.....	.....	.....
4	June 29........	+ 2.5	+1.7	+ 0.8	+16.4	+16.8	−0.4	.....	.....	.....
5	June 30........	+ 1.2	+1.7	− 0.5	+16.5	+17.0	−0.5	.....	......	.....
6	July 1........	+ 4.4	+1.8	+ 2.6	+17.6	+17.1	+0.5	.....	.....	.....
7	July 12........	+ 2.1	+2.0	+ 0.1	+20.4	+19.0	+1.4	.....	.....	.....
8	July 13........	+ 4.5	+2.0	+ 2.5	+19.2	+19.2	0.0	.....	.....	.....
18	July 20........	+ 0.4	+2.1	− 1.7	.....	+20.9	.....	.....	.....	.....
19	July 20........	− 0.4	+2.2	− 2.6	.....	+20.9	.....	.....	.....	.....
20	July 23........	+ 1.5	+2.2	− 0.7	+16.1	+21.8	−5.7	.....	.....	.....
21	July 24........	+ 3.9	+2.3	+ 1.6	+18.0	+22.1	−4.1	.....	.....	.....
22	July 25........	+ 3.5	+2.3	+ 1.2	+22.1	+22.5	−0.4	.....	.....	.....
23	July 26........	+ 0.6	+2.4	− 1.8	+24.6	+22.6	+ 2.0	.....	.....	.....
24	July 27........	+12.5	+2.4	+10.1	+25.2	+23.1	+ 2.1	.....	.....	.....
25	July 28........	+ 4.2	+2.5	+ 1.7	+23.9	+23.7	+0.2	.....	.....	.....
26	July 29........	+ 5.9	+2.5	+ 3.4	+25.8	+24.0	+1.8	.....	.....	.....
27	Aug. 11........	+ 0.6	−3.7	− 3.1	+34.9	+33.4	+1.5	.....	......	.....
28	Aug. 12........	+ 1.2	+3.8	− 2.6	+36.1	+34.1	+2.0	.....	.....	.....
29	Aug. 13........	+ 2.0	+4.1	− 2.1	+43.6	+36.1	+7.5	+43.6	+42.4	+1.2
30	Aug. 14........	+ 2.2	+4.2	− 2.0	+43.1	+36.4	+6.7	+43.1	+42.7	+0.4
31	Aug. 15........	+ 2.9	+4.6	− 1.7	+42.6	+39.5	+3.1	+42.6	+45.8	−3.2
32	Aug. 18........	+ 1.4	+5.9	− 4.5	+54.3	+46.3	+8.0	+54.3	+52.5	+1.8

(If the last four measures show a third tail, the value of the repulsive force for the secondary tail would be 1.91 times gravity, and the the values O_2—C_2 would be slightly changed.)

The agreement of results is fairly satisfactory. Errors of observation[1] in measuring the position angles of so faint and ill-defined an object as a widespread comet's tail must be quite large.

The light-pressure theory makes it plain why the angles between the radius vector and the tail continually increase up to perihelion. This explanation is simpler than to imagine, with Mr. Albrecht, that "this increase may be due to the fact that the speed of the nucleus in the orbit was rapidly accelerated up to the date of perihelion passage, August 27, thereby causing this tail to lag behind more and more."

[1] Mr. Albrecht has informed me that the "published position angles of the tails are the means of two entirely independent sets of measures. The first set was made before the position angles of the radius vector were computed, and, intentionally, three weeks of time were allowed to eclipse before making the second set, so as to be influenced as little as possible by the first measures in judging the directions of the tails."

However, there seems to be a lagging even behind the direction given by the repulsive force, as is seen from the August measures on the principal tail; or, in other words, the value of the repulsive force may increase as the comet approaches the Sun. This increase, shown by the values derived, may be apparent only, being due to errors of observation in determining the position angle of the tail; but it is more likely that it is, in part, at least, real. There is then some force other than light-pressure at work, and this force undoubtedly comes from the more violent action of the gases liberated as the comet approaches the Sun.

On July 24 an interesting break was observed in the tail, and on this night two photographs were taken at the Yerkes Observatory, one at Nanterre, France, and one at the Lick. From measures of Yerkes and Nanterre photographs, Professor Barnard[1] determined a motion of recession of 7 miles per second relative to the Sun; and Mr. Albrecht, by measures on a slightly different portion of the detached area, from combinations of all four photographs, found the recession to be at the rate of 13 miles per second from the Sun. On this date, as is determined from the comet's orbit, the velocity of the head relative to the Sun was 22 miles per second, and of the tail 407 miles per second; this velocity, it may be noted, is independent of the nature of the force emanating from the Sun, whether light-pressure, electrical, or some other. As the motion of the detached portion of the tail was not that of the cometary particles forming the tail, it must be due to perturbations of these motions by meeting with a meteor swarm, or what is more likely, it is caused by a change in the rate of emission of the particles forming the tail.

The light-pressure theory seems to explain most easily the observed phenomena of the tails of comets. The above determined values of the magnitude of the repulsive forces give for the size of the small particles forming the three tails 0.1μ, μ, 1.33μ.[2]

Columbia University,
May 15, 1904

[1] Astrophysical Journal, **18**, 213, 1903.

[2] Mitchell, U. S. *Monthly Weather Review*, July 1904.

MINOR CONTRIBUTIONS AND NOTES.

ESCAPE OF GASES FROM ATMOSPHERES.

A LETTER under the above heading, by Mr. S. R. Cook in *Nature* of March 24, puts forward views which ought not to remain on record without reply; and as between thirty and forty years ago I carried on the investigation into the rate at which gases can escape from atmospheres in the same way as Mr. Cook has done, and arrived from the premises employed by him at substantially the same conclusions, perhaps the best answer will be to state the considerations which led me to distrust that line of argument, and finally to abandon it. To do this, however, requires more to be said than can be brought within the compass of a letter to a weekly journal; and on this account and because the discussion is a physical discussion and concerns one of nature's greater operations, I venture to request the editors of the ASTROPHYSICAL JOURNAL to give space to the following pages, which are substantially as they appear in the current number of the *Philosophical Magazine.*

A study of the phenomena attending the escape of gases from atmospheres has been approached in two ways—*inductively*,[1] by arguing upward from events which are found to have occurred or to be in process of occurring in nature; and *deductively*,[2] by drawing inferences from the supposition that it is legitimate to attribute to the real gases of nature behavior which it has been ascertained would prevail in certain models of gas, so much simpler in their constitution than real gases that the progress of events within them is susceptible of mathematical treatment.

The two methods, as hitherto employed, have led to contradictory results, of which one at least must be erroneous. Mr. Cook, who has of recent years employed the deductive method, expresses the opinion in his letter that the numerical results which have been arrived at by this method

[1] G. JOHNSTONE STONEY, "Of Atmospheres upon Planets and Satellites." *Scientific Transactions of the Royal Dublin Society*, **6**, 305, October 1897; or ASTROPHYSICAL JOURNAL, **7**, 25, January 1898.

[2] S. R. COOK, "On the Escape of Gases from Planetary Atmospheres according to the Kinetic Theory." ASTROPHYSICAL JOURNAL, **11**, 36, January 1900.

G. H. BRYAN, "The Kinetic Theory of Planetary Atmospheres." *Philosophical Transactions*, A. **196**, 1, March 1900.

"will have to stand" until they can be disproved "by other *a priori* reasoning." Serious students of nature must, I think, hold that man, in his dealings with nature, is not in a position to limit in this way the kind of proof he will accept; and that it is sufficient if *in any way* Mr. Cook's inferences from Maxwell's researches can be disproved, whether by valid *a priori* or by valid *a posteriori* reasoning. And, moreover, that when once they are disproved we are brought face to face with the fact that there has been a mistake somewhere in the data which has led those who trusted in them to a false conclusion.

What convinced me several decades ago that the conclusion at which I arrived, and at which Mr. Cook has arrived, is false, is that it represents the Moon as incompetent to get rid of the atmosphere which it originally shared with the Earth, and of the gases which it has since evolved in abundance from its own interior. We knew thirty-five years ago, as we know now, that any reasoning which makes out that the Moon has retained its atmosphere must have a flaw in it somewhere. Furthermore, since that time, other facts not then known have come to light, and in a marked degree confirm the judgment which was then formed. Our confidence that we are on the right track is justifiably strengthened, when, as in this case further discoveries as they emerge confirm the view to which we had been led when our materials were more scanty. The presence of helium on the Earth was not then known; and the argument,[1] which has been based on what is now known of its behavior may be summarized as follows: Helium is supplied to the Earth's atmosphere through certain hot springs, and under circumstances which indicate that it also oozes up through the soil. It is, however, what is carried up by the water of these springs that can be subjected to experimental examination. The other gases of our atmosphere, such as nitrogen, oxygen, and argon, are found to accompany the helium in those springs; but with this marked difference, that whereas the other gases are present in such proportions as are consistent with their merely being portions of those gases which are being returned to the atmosphere after having been washed down into the Earth from the atmosphere by rain, the case is entirely different when we come to helium. The quantity of helium passed into the atmosphere through those springs is found to be from 3000 to 6000 times more than can be accounted for as a return to the atmosphere of helium which had been washed down out of it. Accord-

[1] The argument here summarized is based on the marvelous determinations made by Sir William Ramsay, or in his laboratory, and will be found with the necessary details in a paper "On the Behavior of Helium in the Earth's Atmosphere," by G. JOHNSTONE STONEY, ASTROPHYSICAL JOURNAL, **11**, 369, 1900.

ingly we are justified in regarding this great surplus of helium as being an addition which is being uninterruptedly made to the atmosphere. Notwithstanding this, the quantity of helium in the atmosphere has not gone on increasing. The Earth at the present rate of supply furnishes in a small number of years a quantity of helium equal to the quantity which the atmosphere can at present retain—*i. e.*, in a number of years which is exceedingly small from a geological standpoint, which is the point of view that is here appropriate. The inference from these facts is the obvious one, that helium is by some agency being eliminated from our atmosphere as fast as it is being introduced into the atmosphere from the Earth. Two possible agencies for the elimination of the helium suggest themselves: chemical reactions and an escape of helium from the upper part of the atmosphere. Of these, chemical agency is excluded by the extreme chemical inertness of helium. What remains then is that there is an outflow of helium from the top of the atmosphere equal to the inflow at the bottom, and that the trace of helium which is at any one time present in the atmosphere is helium part of which is slowly making its way upward to the situation from which some of its molecules can escape, and so produce that outflow which balances the net influx at the bottom of the atmosphere.

Having satisfied myself that the deductive method as I applied it (and as Mr. Cook has applied it) lands us in erroneous results, I set to work to scrutinize the data of the deductive argument with a view to ascertaining how far they may be depended upon, and at what points they are doubtful. All branches of physics require us to be more or less on our guard against trusting without sufficient scrutiny to inferences from that mixture of theory and hypothesis of which we are obliged to make use in order to be able to employ mathematics in physical research. The demand for this caution becomes a pressing one when, as in gases, we are obliged to deal with immens numbers of events, *each of which has its own dynamical history with incidents peculiar to itself*, and where what chances on some of these occasions differs enormously from that which occurs in most of them. Of this kind are the interactions between the molecules of a gas and the interfused ether, and especially those complicated struggles between molecules which we call their encounters—events each of which, when viewed, as it ought to be viewed, from the molecular standpoint, is a battle lasting a long time, as time has to be measured in molecular physics, and with an immense number and variety of incidents. These—the interactions between the molecules and the ether, and the interactions between molecule and molecule—are the primary events, the real determining events, which occur within a gas; while the movements of the molecules as

they dart about between one encounter and the next; the spectrum radiated by the gas; the ions which present themselves after some of the encounters; the compounds which result from chemical reactions during some of the encounters (if what we are dealing with happens to be a mixture of suitable gases); and finally that remarkable partition of energy between the events going on within the molecules and the translational motions of the molecules, which is effected during some of the encounters—all of these are subordinate events depending upon those which are above spoken of as the primary events. When dealing with such almost immeasurably intricate and obscure operations of nature, it behooves us with the very utmost caution to distinguish between what is theory and what hypothesis in the data we employ, in order to be able to ascertain how far any conclusions we draw follow from the one, and how far they involve the other with the risks inseparable from it.

Theories are suppositions we hope to be true; hypotheses are suppositions we expect to be useful. As to theories, they are either correct or erroneous. They may be, they usually are, but they by no means need be, of use to man. The virtue of a theory is simply to be true. On the other hand, hypotheses usually makes use of machinery which we can see to be simpler than that operating in nature; and especially is this the case with those hypotheses to which we are obliged to have recourse in mathematical investigations, which, in order to be of use, must be so great a simplification of the complex intricacies of nature that human mathematics shall be able to cope with them.

The *theory* of gas universally put forward in scientific books when the present writer was young was the erroneous statical theory that the molecules of a gas may be stationary, that they have a capacity for expanding and contracting, and that each molecule presses against its neighbors. An illustration frequently made use of in those days was that of a froth of bubbles pressing against one another. This erroneous theory held the field in Avogadro's time and for more than thirty years afterward; but in the fifties of the nineteenth century it was gradually, though not without protest, displaced (chiefly through a masterly series of papers by Clausius) by the kinetic theory, which is now the prevalent theory. The kinetic theory of gas, as formulated by Clausius, regards the molecules of a gas as missiles of equal mass, darting about in space and not acting *sensibly* on one another except when "encounters" chance to take place; *i. e.*, not until the centers of mass of two molecules get within an interval of one another which is less —usually much less— than the average length of the free paths which the molecules describe between the encounters; which free paths are accordingly

approximately straight and pursued with unvarying speed, except so far as they may be slightly influenced by gravity or other external cause, or by some excessively minute part of the interactions between molecules, if any such survives when the interval between molecules gets beyond what we may call their encountering distance.

This is the kinetic theory of gas as put forward by its founder,[1] and any system of bodies which conforms to this definition may be called *a kinetic system.* Thus, there are in nature as many kinetic systems as there are distinct gases; and, moreover, all those models of gas in which the progress of events has been studied by mathematicians are *each of them* a kinetic system. So also are the cosmic bodies of celestial space, if we eliminate from the definition the condition that the masses must be equal; and, in fact, some modification of this clause of the definition is essential even as regards gases, inasmuch as in all the gases of nature there are found some of the missiles differing in mass from others—thus, in diatomic gases ions present themselves with masses that seem to be half the mass of average molecules.

We may add further details without trespassing beyond the domain of theory, *i. e.*, while still endeavoring to describe events as they occur in nature. Thus we may add that elaborate internal events are going on within all these missiles, which internal events absorb about one-third of the whole available energy of the gas; and we know that two partitions of energy take place—one a partition of energy (which probably goes on uninterruptedly) between these internal events of the molecules and the events of the ether, the other a partition of energy which now and then occurs with comparative suddenness between the internal events of the molecules and their translational motions. This latter transfer of energy seems to take place only when two molecules are in grip with one another during an encounter, and not at every encounter, but only during those which take place under certain necessary conditions. If, as seems probable, encounters with these special characteristics are as rare as those which result in the breaking down of molecules into ions, or of those which result in chemical reaction in a mixture of equal volumes of chlorine and hydrogen, then the infrequency of their recurrence can be estimated; and, in cases in which it has been found possible to make the estimate, the infrequency seems to range

[1] Clausius' papers were preceded by a paper by Waterston which was presented to the Royal Society in 1845, but which was not then published. This paper, when it long afterward came to be printed, was found to contain a most valuable anticipation of the kinetic theory as developed by Clausius. If Waterston's paper had been printed in due course, the kinetic theory would probably have been adequately dealt with some years sooner.

from one out of 10^9 encounters down to about one in 10^{15}, when we pursue the observations so far as they have been recorded.

It is here that I strongly suspect, though I am not in a position to claim that I know, that the mistake has been made by Mr. Cook, and by my friend Professor Bryan, who both tacitly assume that this partition of energy is a process which goes on uninterruptedly, even in the upper parts of the atmosphere. Whether the mistake be here or elsewhere may as yet be only highly probable; but that a mistake exists *somewhere* in the premises of the deductive argument was placed beyond question by nature when she presented to us events that have occurred, or are occurring, which negative some of the inferences to which those data lead. We may be unable with certitude to put our finger upon the precise spot where the mistake came in, but that a mistake has come in somewhere can be proved.

When Maxwell determines his law for the distribution of speeds in a kinetic system, he exercises a caution[1] which has not always been observed by his successors, and is careful to present the law as the law governing the distribution of speeds (not in every, or indeed in any, gas, but) in a kinetic system which consists of numberless equal particles, each of which is a perfectly rigid and perfectly elastic sphere, after an immense number of collisions have taken place—assumptions which he afterward varied in different ways, as by substituting particles of other forms, or points repelling one another inversely as the fifth power of the distance. The several assumptions which he thus makes are put forward, not as theory, but as hypothesis; they do not profess to reproduce any existing gas, but substitute for the gas an artificial model; and Maxwell is careful to keep this prominently before the mind of his reader.

As to his exponential law for the distribution of speeds, it is the solution of a functional equation, which in turn is the expression of the assumption that the number of molecules whose velocities lie between u, v, w, and $u+\delta u$, $v+\delta v$, $w+\delta w$ *must be some junction oj* u, v, *and* w. Now, this is true of Maxwell's models, but cannot be the case in any gas in which there is an irruption of energy from the internal motions to the translational on the occurrence of events which depend either wholly or partly on conditions other than the mere translatory speeds of the molecules—such conditions, for example, as the aspects of the two molecules to one another when the encounter is about to take place, or the phases at which the internal motions had arrived at that instant of time, or many other conditions that are possible and can easily be conceived. Accordingly, whenever a mathematician applies Maxwell's law under the impression that, as regards any

[1] See MAXWELL'S *Scientific Papers*, I, 380, or *Phil. Mag.* for January 1860.

particular gas, it is more than an approximate law, he tacitly assumes either that there are no internal events (as in Maxwell's models), or that, if there be internal events (as in all real gases), the partition of energy between these internal events and the translational motions is a transfer taking place at such short intervals that it may legitimately be treated by the mathematician as a process which goes on continuously and at a constant rate. At the bottom of our atmosphere an event that happens once in 10^9 encounters occurs to each molecule as often as seven or eight times per second. Even here the assumption that the transfer of energy goes on uninterruptedly makes but a rough approximation to the truth, and it is utterly remote from being an approximation in that penultimate stratum[1] of the atmosphere from which nearly the whole escape of molecules takes place, and especially in regard to an event like the escape of a molecule from the Earth, which is mainly the outcome of the circumstance that an individual encounter has chanced to be very unlike ordinary encounters. Hence, in no real gas can the actual law of the distribution of speeds be *identical* with Maxwell's exponential law, nor with any of the exponential laws of Maxwell's successors; although under the conditions which prevail in our laboratories these laws may be an approximation sufficient for many useful purposes.

The cases in which Maxwell's approximate law may legitimately be employed can be pointed out. Whenever an approximate law presents itse f in an exponential form with negative index, the approximation holds good as an approximation over that small part of the range where the exponential function acquires large values, but can no longer be depended upon as an approximation in regard to the parts of the range where the exponen-

[1] If we conceive the helium in the Earth's atmosphere to be divided into horizontal strata A, B, etc. . . . . X, Y, Z, beginning at the bottom of the atmosphere, then Z the outermost of these, may be defined as the layer which is characterized by the presence of helium molecules which, having been thrown up from below, usually — say, in nine cases out of ten —fall back without having met with an encounter in stratum Z, Y, the penultimate stratum, may be defined as that layer within which most of the molecules thrown up into the ultimate stratum, Z —say, nine out of ten of them — had met with their last encounter, and within which most of those which fall back from stratum Z will meet with their next subsequent encounter. Molecules flying upward after an encounter in the next underlying stratum, the ante-penultimate stratum X, will occasionally, although but seldom, escape encounter in stratum Y, and so get direct from X into Z.

Accordingly, the molecules which succeed in escaping from the Earth must have come in most cases direct from a favorable encounter within the penultimate stratum Y, in a few cases from an encounter in either X or Z, and but very seldom indeed from any of the strata farther down than the ante-penultimate stratum X. These outer strata, especially Y and Z, must be of immense depth.

tial function is small. Maxwell makes a legitimate use of his law when, through its instrumentality, he discovered his remarkable explanation of viscosity and diffusion, and investigated the laws of those phenomena. In reference to these, what happens in he case of velocities which are infrequent is of small account; but the application made by Professor Bryan and Mr. Cook is to the rare events which occur within that part of the range where the approximation breaks down, and where, in consequence, the exponential law is misleading. It is this oversight to which I think it likely that we are mainly to refer numerical results which are found to clash with events that hav taken place or hat are taking place upon the Moon and the Earth.

The inquiry in which I engaged in the sixties of the last century led also to the de ection of other defects in the premises made use of by those who have trusted in the deductive method. One of these concerns the ambiguities which surround the use of the term "temperature." Temperature is not one physical measurement, but two groups of physical measurements, essentially different according as we test equality of temperature by there being no transfer of heat *by conduction* when two bodies are brought into contact, or *by radiati n* when they are made to stand apart. This establishes a division of temperatures into two principal groups, and these groups require further subdivision. The temperature of a body determined in these two different ways may be called its conduction temperature and its radiation temperature; of each of which there are several varieties. There are accordingly many different kinds of temperature. In the case of gases, conduction (including convection) is mainly concerned with the translational speeds of the molecules, while radiation in the first instance affects only the internal events going on within the molecules. In most laboratory experiments carried on as they must be at the bottom of our atmosphere) the partition of energy between the internal events of each molecule and its translational movements takes place so frequently– probably several times every second in a gas at standard temperature and pressure—that the distinction even between the two main kinds of temperature does not need to be attended to. But, to go to the opposite extreme, let us consider the case of a gaseous molecule which has escaped from the Earth and travels like an independent planet through space. Here no interchange of energy can take place between the translational movement of the molecule and its internal events. Under suitable external influences either of them may be made to vary to any extent without this affecting the other. The two kinds of energy, or if we please so to call them of temperature, have become divorced; and intermediate stages between these extremes would be found

to exist within an atmosphere if we could explore it from its bottom to its top.

Further distinctions have to be made within the two principal kinds of temperature. Those which have to be taken into account in the present investigation are the varieties of radiation temperature. A body, like the Sun, acting by radiation upon different gases has no one definite radiation temperature, but may be at a different radiation temperature in regard to each gas. Thus, the Sun is hotter with regard to the helium of the Earth's atmosphere than with regard to its hydrogen. This we know, because the radiations from the Sun which can affect hydrogen come in the form of the rays corresponding to the hydrogen lines of the solar spectrum which are dark, while the radiations which raise the temperature of helium come through rays corresponding to the helium lines, of which the principal one within the visible spectrum—the double line D_3—is as bright as the neighboring part of the spectrum. Hence the radiation which reaches helium in the outer part of our atmosphere has the full intensity of radiation from the Sun's photosphere.

Reviewing the whole case, we find that in the stratum of the Earth's atmosphere from which helium escapes, the opportunities for exchanging energy between the internal motions and the translational, instead of occuring to each molecule several times per second, may be so infrequent that they occur only once in several hours. During all its intermediate flights the molecule is exposed during the daytime to the full glare of radiation as intense as direct radiation from the Sun's photosphere. In this way the internal motions of the molecule will be kept for some hours excited to intense activity, and if during these hours that special kind of encounter happens to take place which affords an opportunity for an interchange between the internal and translational energies, the two encountering molecules will fling asunder with what may be described as explosive violence. All that is then necessary for a molecule to escape is that one of the two that have encountered shall have the direction of its flight outward, that it shall have sufficient speed, and that it shall escape other encounters. If the chance that these events shall concur befalls each molecule in the penultimate stratum of the helium atmosphere as often as once in several days, there would probably be an abundant outflow of helium from the Earth to account for the observed rate of its escape.

Here, however, we are on debatable ground. We can only follow the individual events of molecular physics with probability, not with certainty. But, on the other hand, when we trust to the inductive argument based on the ascertained behavior of helium, as stated in an earlier paragraph, *we are on secure ground.* We may rely on the conclusion to which it leads,

viz., that helium *is* escaping from the Earth's atmosphere, and that the rate of escape is the same as the rate of the net inflow from the Earth into the atmosphere. By the net inflow is meant the supply after deducting something like a 1–6000 or 1–3000 part of the whole, in order to allow for the very minute quantity of helium that had been washed out of the atmosphere by rain and which is being restored to it.

There are other matters, too, which would need to be understood and allowed for before we should be entitled to trust the deductive method of proof. Thus, the internal events that go on within the molecules of matter are of more than one kind, and in gases stand differently related to the translational motions. This is revealed to us by phosphorescence and other phenomena. An attempt to make a preliminary classification of these internal events has been made by the present writer in a memoir on the kinetic theory of gas.[1] But without going into these and other matters enough has been said to show how inadequate the deductive method is—at least as hitherto handled—to be a safe guide in dealing with the matters with which it has been made to grapple. This of course also shows that objections based on investigations of this character have no weight against the testimony about the rate at which gases do actually escape from atmospheres which is given by such *facts* as the absence of atmosphere from the Moon and the behavior of helium upon the Earth.

The objection urged by Mr. Cook against accepting the inductive proof of the actual rate of escape of gases from atmospheres is analogous to the objection urged by some scientific men when in 1867 I brought forward a proof[2] that in an atmosphere of mixed gases the atmosphere of each gas must have a different limit, the lighter constituents overlapping and extending beyond those which are denser "Oh!" it was then said, "that can't be the case. It is inconsistent with Dalton's law of the equal diffusion of gases." Yet I have lived to see my conclusion generally, I believe universally, accepted by physical astronomers; and I look forward with some hope to an ultimate acquiescence in what is now being objected to, in reference to the escape of gases from atmospheres. In both cases the objection rests on the same error—the mistake of hypothesis for theory, and the consequent mistake of a law which is approximate for a law of nature.

G. JOHNSTONE STONEY.

30 LEDBURY ROAD, LONDON, W.,
May 12, 1901.

[1] "Of the Kinetic Theory of Gas regarded as Illustrating Nature," *Scientific Proceedings of the Royal Dublin Society* of June 1895, **8**, 356, or *Phil. Mag.*, October 1895, p. 362.

[2] "On the Physical Constitution of the Sun and Stars," *Proc. R. S.*, No. 105, p. 1 (1868). See especially paragraphs 23, 24, 25.

NOTE ON THE PARALLAX OF *NOVA PERSEI*.

In the March number of this Journal Mr. Otto Luyties asserts[1] that my computation of the parallax of *Nova Persei*[2] is incorrect, because "it involves two tacit assumptions, of which one is erroneous." But the assumptions named were not made by me, even tacitly, since my points of reference were not taken at "the outer limits of the apparent illumination." The positions actually chosen were at centers of greatest intensity, where the objections made do not apply.

Consequently my conclusions as to the parallax of *Nova Persei* and as to the relative masses of the ions are in no wise invalidated by a correct application of the principle elucidated in Mr. Luyties' article.

Frank W. Very.

Westwood, Mass.,
June 3, 1904.

[1] Astrophysical Journal, **19**, 130, 1904.

[2] *American Journal of Science*, **16**, 127, 1903.

NOTICE.

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention will be given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed.

Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

If a request is sent *with the manuscript* one hundred reprint copies of each paper, bound in covers, will be furnished free of charge to the author. Additional copies may be obtained at cost price. No reprints can be sent unless a request for them is received before the JOURNAL goes to press.

The editors do not hold themselves responsible for opinions expressed by contributors.

The ASTROPHYSICAL JOURNAL is published monthly except in February and August. The annual subscription price for the United States, Canada, and Mexico is $4.00; for other countries in the Postal Union it is 18 shillings, 6 pence. Correspondence relating to subscriptions and advertisements should be addressed to *The University of Chicago, University Press Division, Chicago, Ill.*

Wm. Wesley & Sons, 28 Essex St., Strand, London, are sole European agents, and to them all European subscriptions should be addressed.

All papers for publication and correspondence relating to contributions and exchanges should be addressed to *Editors of the* ASTROPHYSICAL JOURNAL, *Yerkes Observatory, Williams Bay, Wisconsin, U. S. A.*

THE
ASTROPHYSICAL JOURNAL
AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME XX SEPTEMBER 1904 NUMBER 2

ON THE OXYGEN ABSORPTION BANDS OF THE SOLAR SPECTRUM.

By O. C. LESTER.

SPECTROSCOPISTS have long taken a special interest in those peculiar bands of the solar spectrum of which B is a typical and familiar example. Singly and collectively they have formed the subject of many investigations, especially since instruments of research began to be powerful enough to reveal, at least in some degree, their peculiar structure. The early researches of Egoroff,[1] Janssen,[2] and Thollon[3] had for their chief aim the discovery of the element to which these bands were to be attributed, with the result that they were shown to be due, at least in part, to the absorption of solar energy by the dry oxygen of the Earth's atmosphere. The later work of Cornu,[4] Janssen,[5] Dunér,[6] and others furnishes almost conclusive evidence that they are due solely to this cause. After the introduction of still more powerful and perfect spectroscopes, attention was drawn to the remarkable geometrical structure of each of these bands and to their striking similarity. From the latter point of view the most

[1] *C. R.*, **92**, 385, 1881; *ibid.*, **97**, 555, 1883.

[2] *Ibid.*, **107**, 1889; several communications.

[3] *Jour. de Phys.*, (2) **3**, 1884.

[4] *Ann. de Chim. et Phys.*, **7**, 5–102, 1886.

[5] *C. R.*, **107**, 672, 1889.

[6] *Ibid.*, **118**, 1894.

important studies have been those of Cornu,[1] Langley,[2] Piazzi Smyth,[3] Higgs,[4] and Deslandres.[5]

Langley and Smyth point out only the general resemblance as seen from their early photographs and drawings. Cornu attempted to establish certain more definite relations, but his wave-lengths were not measured very accurately and the relations to which he called attention were only roughly approximate. Higgs confines himself to the study of the relations of the lines in a single band, taking B as an example, and shows that if the positions of the lines in any series are laid off in wave-lengths along the X-axis and lines drawn through them parallel to Y, while on Y are laid off equal divisions and lines drawn parallel to X, then the intersections of the two systems of lines lie very closely on a parabola whose equation is of the form

$$\lambda = V + \frac{(n+c)^2}{p} ,$$

where n is any number of units from the vertex of the curve, and V, p, and c are constants. There are two parallel parabolas corresponding to the two series in each band, the vertices coinciding with the beginning of each series.

Deslandres points out a general similarity between A, B, and α, and certain bands discovered by Huggins, Liveing and Dewar, and himself in the ultra-violet spectrum of water-vapor. The water-vapor bands reproduce the oxygen bands on a larger scale, the ratio of the corresponding A groups, for example, being about 12 to 1. The chief differences are that the water-vapor groups as given by Liveing and Dewar[6] show no separation of head and tail, and, according to Deslandres,[7] are composed of two series instead of one. It will be seen later that the analogy is even closer than Deslandres supposed, as these two differences do not really exist.

One purpose of the present research was to investigate as fully as possible the relations existing between the lines of a band and between the several bands, taking into account those groups above

[1] *Ann. de Chim. et Phys.*, **7**, 5–102, 1886.

[2] *Proc. Amer. Acad.*, **14**, 62, 1878.

[3] *Madeira Spectroscopic*, 1881.

[4] *Proc. R. S.*, **54**, 200; also *Astronomy and Astro-Physics*, **12**, 547, 1893.

[5] *C. R.*, **100**, 854, 1885.

[6] *Phil. Mag.*, **26**, 286, 1888.

[7] *C. R.*, **100**, 854, 1885.

a which do not seem to have been considered before. In order to do this satisfactorily it was necessary to have the wave-lengths measured very accurately, and on examination it did not appear that there had been made by any one observer a complete and accurate measurement of the whole spectrum. Cornu,[1] in 1886, made extensive measurements on A, B, and *a*, but with his apparatus, although he had a good grating, it was difficult to get a precision greater than 0.02 where unity was 10^{-6} mm; whereas at present it is possible to measure many of the lines with an accuracy greater than 0.01 where unity is 10^{-7} mm, *i. e.*, with a precision twenty times as great. Cornu's error would be 0.2 of a unit on Rowland's scale, an amount, as will be seen later, almost equal to the total variation upon which Deslandres' first law is based. Consequently, Cornu's measurements were not available in the present work. The best determinations previously made are those of Rowland and Higgs, but neither gives all the lines even of the groups A, B, and *a*. Rowland's measurements are nearly complete for B, but he gives few for the other groups. Higgs gives A and B and up to the ninth pair of *a*, and although he and Rowland agree remarkably well in general upon B, judging from the few lines in A which both have measured, the agreement is not so good, there being much greater discrepancies than one would expect from the accuracy claimed for their measurements. Hence it seemed worth while to make new determinations of all, or nearly all, the lines previously measured; in addition many are given which have not been measured before, so far as the writer is aware. It is hoped, therefore, that the present determination of the wave-lengths, taking into account the best previous results, has both extended and unified the measurements on these bands and rendered them on the whole more accurate, thus doing for the absorption spectrum of oxygen what the similar work of M. Eisig[2] has done for the line spectrum.

Because of the precision which it is possible to obtain in the measurement of this spectrum, a careful study of the relations subsisting between the lines and bands furnishes an excellent test of the

[1] *Ann. de Chim. et Phys.*, **7**, 5–102, 1886.

[2] "Das Linienspektrum des Sauerstoffs," *Wied. Ann.*, **51**, 747, 1894.

exactness of the so-called laws of Deslandres[1] for band spectra. They may be stated briefly as follows:

I. In a given band the intervals from one line to the following in any series, calculated in vibration-numbers, are in arithmetical progression: *i. e.*, the lines are connected by a relation of the form

$$1/\lambda = N = a + bn^2,$$

where a and b are constants and n takes on all integral values from 0 to n.

II. When two or more series arise from the edge of a band, they are similar in all respects, and all bands belonging to the same substance have the same number of series.

III. In a series of bands the vibration-numbers of the edges form a series similar to that of the lines in a single band, or

$$N = A + Bm^2.$$

These laws are the most general in their application that have yet been announced. Deslandres tested them upon many spectra, including the subject of the present study, but he has not published details showing the exactness of the agreement which he obtained.[2] Kayser and Runge[3] have obtained a general confirmation upon bands of many substances, including those of N, C, CO, CN, and I, but the laws do not apply equally well to all cases, and occasionally appear to degenerate into mere interpolation formulæ.[4] The difficulties in the way of obtaining more exact expressions for the laws are in measuring the wave-lengths of bands accurately enough to warrant taking account of small variations in the reciprocals, and, in the case of the third law, in finding a long enough series of bands capable of precise measurement. The most exact measurements by which the third law has been tested are those of Kayser and Runge on the band spectrum of CN. But here the longest series has only four bands, which gives but two second differences. Longer series

[1] "Caractères principaux des spectres des lignes et de bandes. Considerations sur les origines de ces deux spectres," *C. R.*, **137**, 1013, 1903.

[2] KAYSER, *Handbuch der Spectroscopie*, **2**, 474.

[3] "Ueber die Spektren der Elemente," *Abhandl. der Berl. Akad.*, 1888–92.

[4] An example in point is cited by KAYSER (*Handbuch der Spect.*, **2**, 470), in the case of the cyanogen band λ 3883.55. This band, however, Deslandres claims to be exceptional.

are obtainable as in the second part of the *N* spectrum from 500 $\mu\mu$ to 2.80 $\mu\mu$ cited by Deslandres in support of his third law, but the measurements are not so exact and the confirmation is only general. Moreover, most of the bands hitherto measured are in the upper part of the spectrum, where a small error in λ causes a large error in the reciprocal.

MEASUREMENTS OF WAVE-LENGTH.

In the measurements of Rowland and Higgs, to which reference is made above, both used the same unit, viz., 10^{-7} mm, and their results appear to be equally accurate. For those lines in B and *a* which both have measured, they agree in general to within 0.01 or closer. Whenever such agreement occurs, the value for the line adopted in the present work is the mean of the two. In some instances the disagreement is greater than one would attribute to errors of observation, and in such cases the value adopted is the mean of my own final result, and the one which it confirms, provided such agreement is decidedly stronger with one than with the other. In some cases the mean of all three measurements was taken. For the large majority of the lines in A and *a* the values given are the results of my own and Higgs' measurements alone. Those for *a′* and *a″* have not been given before. Since 0.01 of a unit is about the limit of accuracy in general, it has been thought best to retain only two decimal places in the wave-length, except in the case of B where many of the lines are taken as Higgs and Rowland give them. It may be added further that the third decimal place of λ rarely affects even the seventh place in the reciprocal.

In respect to the group A, Rowland and Higgs rarely agree more closely than several hundredths. The variations in their measurements of the same lines are anywhere from about 0.01 of a unit to more than 0.1, and are not systematic, and consequently not due to constant errors. These differences are perhaps not so surprising when we consider the difficulties in obtaining good photographs of A and the variable appearance of the lines with the state of the atmosphere. However, with good photographs, such as may be obtained with the best apparatus on clear, calm days with a high Sun, it seems possible to obtain measurements by independent observers which seldom

differ for any line more than 0.02 or 0.03 of a unit from their mean. Assuming this mean to be correct, none of the values given for the lines of the A group should differ by more than this amount from their true value, and most of them should be closer.

My own measurements of the A band were made from photographs taken with a large concave Rowland grating having 20,000 lines per inch and 21.5 feet radius installed in the Sloane Physical Laboratory. A clear, sharp photograph of the second spectrum of the ultra-violet was obtained in a narrow strip through the middle of the same plate, thus affording an excellent method of comparison with Rowland's standard lines in this region (about 3850).

The measurement of B and a is much easier, because the lines are more definite and there are plenty of well-defined standard lines between which to make interpolations. The chief difficulty is with the very weak lines of the last pairs, for which almost any magnifying power of the micrometer is too great. Some such lines were measured by first putting a very fine mark on the back of the plate coincident with the line as seen from above. The measurements of the negatives were checked also by micrometer measurements on Rowland's maps, which gave very good results, care being taken to set on the center of density of the lines; and in the case of very faint lines the latter method is the better. Interpolating between standard lines obviates errors in the map-scales which are considerable. The precision of measurement for the final values of these two groups should be at least equal to 0.01.

The group a' was first noted by Jewell.[1] Many of its lines are too faint to be measured directly with a micrometer either on the negatives or on the maps. For these a faint mark was used as before. In the "head" or first band of the group many of the lines appear double, and some foreign lines of the same intensity seem to be present. In making out the series for the "head," the line 5789.40, which is the "chief line," corresponding to similar lines in A, B, and a, has been assumed double, as it is in all other cases. The only indication of duplicity actually shown is its greater intensity and a certain flatness in the intensity-curve characteristic of close doubles. Moreover, the regularity of the series calls for a double

[1] *Astronomy and Astro-Physics*, **12**, 815, 1893.

here. The accuracy of measurement for a majority of the lines is about 0.02 tenth-meter.

The positions of many of the lines in a'' were calculated approximately from relations established between the other bands. The observed values differ by less than 0.2 of a unit from those calculated. The lines are all extremely weak. Some, though not all of them, appear on negatives taken in zero weather, which indicates that they are not water-vapor lines. The first band of the group begins as usual with a double line, possesses a chief line, and a final pair in its proper position, as a glance at Figure 1 will show. Probably not all the lines present can be seen. Many are so faint as to be visible only on the charts, and then only when they are held in certain positions with respect to the light, or in such a position that the eye gets the effect of increased density by looking along the line. Some of the negatives were enlarged on ordinary sensitive plates, and two such enlargements superposed, but this did not bring out all the lines. Two or three lines are stronger on the corresponding chart of Rowland's first series, which is considerably more intense, though lacking in definition. No attempt has been made to measure most of the lines of this group nearer than to the nearest half-tenth, nor to arrange the lines in series.

Blunders and mistakes in calculation for all groups except A have been practically eliminated by the use of verniers made to fit Rowland's charts. The verniers were arranged to read directly to 0.04, and by estimation even closer; and, in spite of irregularities in the map-scales, any but very small mistakes could be detected at once. The wave-lengths for the several groups are given in the accompanying table.

The terms "head" and "tail" or "train" used to designate the two parts of the A, B, and a groups cannot be taken in this case in the usual sense of these terms as applied to band spectra, and are really misnomers. The spectrum is composed of two series of entirely separate bands instead of a single series, the so-called "heads" forming the first and the "tails" the second. The first series has the appearance of being nearly all "head" and the second all "tail," but the apparent crowding and confusion in the case of the former is due to the distance between the first few pairs being less than their

width. In all these bands the distance between pairs becomes greater with increasing wave-length, while the width of the pairs becomes less, and these two changes acting in the same direction

TABLE I.

NOTE.—Each band contains two series, which are arranged in pairs. Consequently, to obtain a single series alternate numbers must be taken.

A		B	
First Band	Second Band	First Band	Second Band
7594.00	7621.27	6867.458	6884.080
95.27	23.53	68.457	86.004
94.28	24.77	67.794	86.982
95.55	27.30	68.780 (double ?)	89.183
94.81	28.52	68.337	90.144
96.06	31.28	69.338	92.614
95.55[1]	32.49	69.144	93.559
96.79	35.47	70.130 [3]	96.282
96.51	36.65	70.220	97.197
97.74	39.86	71.180	6900.196
97.70	41.01	71.528	01.116
98.90	44.46	72.480	04.363
99.14	45.59	73.078	05.263
7600.30	49.27	74.030	08.785
00.80	50.40	74.888	09.677
01.95	54.33	75.830	13.449
02.65	55.45	76.953	14.331
03.80	59.62	77.878	18.365
04.73	60.73	79.275	19.245
05.87	65.14	80.173	23.542
07.05	66.25	....	24.416
....	70.89	....	28.986
....	71.07	....	....
08.20	76.86	....	29.839
09.57	77.92	....	34.669
10.72	83.06	....	35.518
12.33	84.11	....	40.584
13.45	89.47	....	41.430
15.32	90.50	....	46.770
16.41	96.11	....	47.580 ?
....	97.13	....	....
....	7703.02 [2]	....	....
....	04.02	....	....
....	10.16	....	....
....	11.16	....	....
....	17.00	....	....
....	18.55	....	....

[1] Higgs gives also 7605.42 and 7695.66. Probably outside edges of this line.

[2] These lines are taken from Higgs' measurements, but, judging from the uniformity of the preceding part of the series, they are a little large.

[3] The close double called the "chief line."

soon reduce the appearance of the first band to a regular arrangement of pairs like the second. That the "head" and "tail" are really separate bands is apparent from the following considerations.

Speaking generally, relations subsisting between the lines and bands of the first or "head" series, analogous to those between the lines and bands of the second or "tail" series, are always of a differ-

TABLE II.

a		*a′*	
First Band	Second Band	First Band	Second Band
6276.81	6287.94	5788.33	5796.30
77.66	89.60	(88.55)[4]	97.76
77.03	90.42	89.00	98.43
		(double ?)	(covered)
77.86	92.35	88.75	5800.18
77.52	93.15	89.40	00.83
		(chief line)	
78.29[1]	95.36	89.40	02.87
78.29	96.14	(89.71)	03.51
79.07	98.64	90.07	05.84
79.31	99.41	90.32	06.47
		(double ?)	
80.08	6302.18	90.97	09.10
80.61	02.95	91.49	09.72
81.37	06.00	(91.78)	12.64
82.16	06.75	92.15	13.25
82.93	10.06	92.96	16.46[5]
....	10.81	93.60	17.07
84.00	14.40	....	20.58[6]
84.75	15.14	....	21.16
....	19.02	....	24.94
....	19.75[2]	....	25.52
....	23.92	....	....
....	24.64	....	....
....	29.10	....	....
....	29.82	....	....
....	34.55	....	....
....	35.26[3]	....	....
....	40.28	....	....
....	40.98	....	....
....	46.27	....	....
....	46.96	....	....

[1] "Chief line" and evidently a close double. Higgs gives also 6278.19 and 6278.38, apparently the outer edges.

[2] End of Higgs' measurements. [3] Hidden by adjacent heavy line.

[4] Lines in parentheses do not appear to fit into the series. Perhaps foreign.

[5] Hidden by heavy adjacent line. [6] Very dim and hard to measure.

TABLE III.

a''	
First Band	Second Band
5377.20	5384.27
77.32	85.45
78.00	86.05
78.38 (chief line)	87.50 (double) ?
79.45 (double ?)	88.10[1]
80.00	89.85, 90.45[2]
80.20 (double ?)	92.55
80.85 (double ?)	93.10[1]
81.40	95.55 (covered)
81.97	96.10

ent order of magnitude and are frequently different in sense. Both "head" and "tail" begin with pairs of almost the same width, which decrease in width with increasing wave-length at slightly different rates. No series in a "head" or "tail" is a continuation of a series in the other, as it should be if they were parts of the same band. In fact, if the head series be continued into the tail, or the tail series extended upward into the head, the calculated pairs of either fall irregularly between the observed pairs of the other. Also the first and second differences between homologous lines in the heads and tails form entirely different series, as do the ratios of the same lines. Further, while there are no lines in the places calculated for the tail series extended upward, faint lines appear to be in their proper places for an extension of the head, just as if the first band, instead of fading out gradually as the second does, should drop very suddenly in intensity on approaching the region occupied by the other. That this is apparently what happens is indicated also by the fact that the last line of what is usually considered the last pair in the "head" of B is scarcely half the intensity of its mate,

[1] Stronger on old series of charts.

[2] Hidden by a group of five heavier lines, none of which actually cover the positions, but the shading renders them invisible.

and in *a* is less than half. It is true that, in general, the first series of the "head" is slightly the stronger, but the change in the last pair is abrupt. A similar difference is noticeable in the corresponding lines of A and *a′*, but it is not so marked. Continuing the series in the first bands of B and *a*, we find the following agreement between observed and calculated lines, the former being dim and somewhat nebulous, but capable of measurement from the charts closely enough to indicate an apparent connection with the band.

TABLE IV.

Continuation of First B Band		Continuation of First *a* Band	
Observed	Calculated	Observed	Calculated
6879.28 } Last pair strong lines	.....	6284.00 } Last strong pair	.....
80.17	.....	84.75	.....
81.80	81.85	86.09	86.11
82.72 (hidden)	82.72	86.88	86.84
84.65	84.67	88.48	88.49
85.54	85.52	89.20	89.20
87.75	87.74	91.14(covered)	91.14
88.60	88.57	....	91.83
91.05	91.06	....	.....
91.87	91.87	....	.....
94.67	94.63	....	.....
95.50	95.42	....	.....

The physical characteristics of these weak pairs follow the general rule of the first bands in that the lines of the first series are slightly stronger. There are other dim lines between the main pairs, but they do not seem to have any connection with the series.

Indications that the first A band is continued beyond the last strong pair are not lacking, although they are not so good as in B and *a*.

Some of the lines in what Higgs calls the "secondary train of A," a series of sharply defined, less intense pairs situated between the main pairs, are in positions suited to the continuation of the head series; but most of them do not fit, unless we suppose the first band not only decreases in intensity on reaching the region occupied by the second, but also at first increases the width of its pairs and again decreases them, in harmony with the series of the second band. But the lines of this group are apparently still more complicated.

TABLE V.

Continuation of First A Band	
Observed	Calculated
7615.32 } Last strong pair	....
16.41 }	....
18.57	7618.53
19.51	19.60
22.05	21.07
....	23.02
25.63	25.64
26.73	26.65
29.38	29.53
30.55	30.51

In addition to the first "secondary train" of Higgs there seems to be a second one, which first makes its appearance just on the more refrangible edges of the lines of the sixth pair, and is visible for a few pairs farther, each succeeding pair being farther removed on the more refrangible side from the corresponding pair of the main series. The series cannot be observed far enough to decide whether it follows laws similar to those governing the others. Traces of similar series appear also in B and to a less extent in *a*.

Since band spectra are due to vibrating molecules or aggregates, we might expect these secondary series not only to occur, but also to be stronger and more numerous in the bands of greater wave-length which are due to the vibrations of the more complex molecules. The vibrations producing the secondary series would be in the nature of harmonics, and the fact that they are always more refrangible seems to indicate this. No attempt has been made to complete any of the secondary series in the present work, as not enough lines have been clearly identified. These series correspond to the less intense, more refrangible series observed by Deslandres in the spectrum of water-vapor.

In his study of the single band by means of the parabola, Higgs shows a smooth curve connecting the lines of the "head" and "tail" series, as if they were parts of the same band; leaving a gap, however, where the two parts are separated. But if the curve is drawn according to his specifications, it shows a fault or offset at the gap, just as we should expect from the foregoing, indicating that the series belong

to similar, but not to the same bands. Further, the distances in wave-lengths between homologous lines for any two "heads" form a decreasing series, while for the "tails" the series are increasing. In fact, from every standpoint the two bands of a group seem to be independent and the spectrum to be composed of two band-series.

The geometrical relations of the various groups are clearly seen in Figs. 1 and 4, which have been drawn from measurements of the wave-lengths.

The first line-series in the first series of bands is slightly stronger than the second, but the reverse is true in the case of the second band-series. Also the width of corresponding pairs decreases regularly from A to a''.

In the first band of B there are ten strong pairs, and in the second thirteen easily measured, with possibly two others which are too faint to admit of measurements accurate enough to decide whether they belong to the series or not. In the second band the rate of decrease in intensity changes suddenly at the eleventh pair. There are ten strong pairs in the "head," and from analogy we should expect a sudden decrease with the eleventh, and this is in harmony with what has been said above with respect to the continuation of the series.

The first band of a has eight strong pairs, and the second fourteen pairs. The same abrupt change in the intensity of the second band is noticeable in the eighth pair, though it is not so strong as the corresponding change in B. Likewise the lines indicating a continuation of the first band of a are not so weak as in B, again preserving the analogy. In A there are fourteen strong pairs in the first band and eighteen pairs in the second. In a' there are six in the first and nine in the second.

In most spectra it is the vibration-numbers which are subject to regular laws rather than the wave-lengths, but in this case it makes little difference which is taken so far as Deslandres's first law is concerned. This is due to the fact that none of the bands is very extended. The first law is roughly approximate in all the bands, but it does not hold to the degree of precision with which the measurements are made. The agreement between the observed values and those calculated by Deslandres' formula,

$$N = a + bn^2 \ ,$$

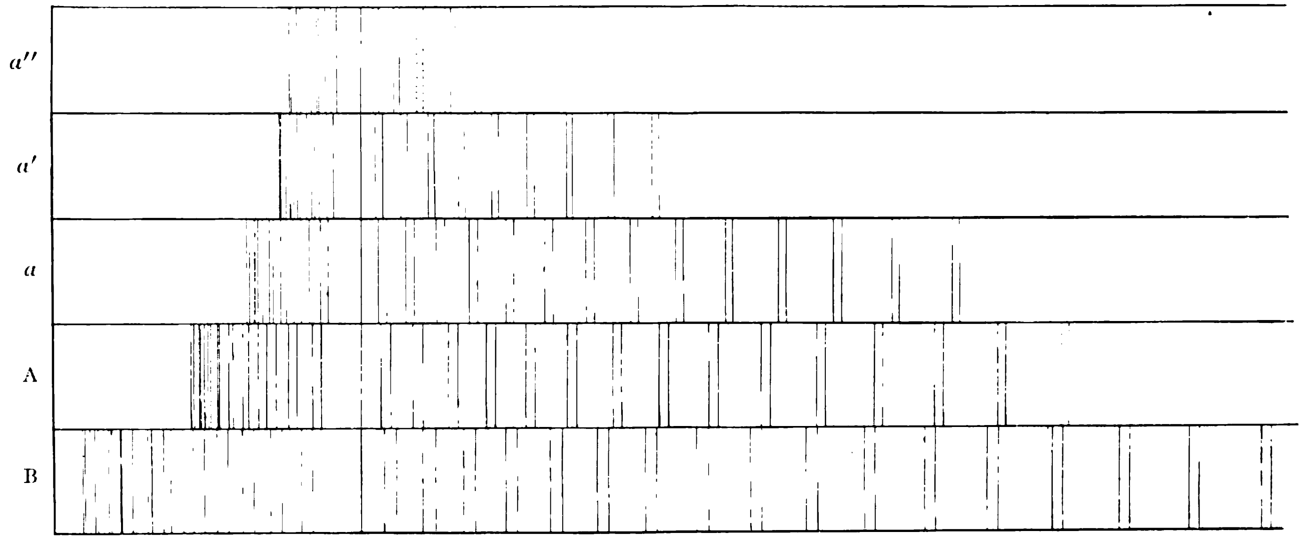


FIG. 1.—The groups as represented in Fig. 1 are drawn from the measurements of the wave-lengths. Beginning with A they are arranged in order, one above the order, with the first lines of the second bands forming a straight line. This pyramid-like grouping of the bands is especially effective in showing the symmetrical arrangement of the pairs from group to group.

is illustrated in the following applications to the first series of the first band of A and to the second series of the second band of B. The constant b is calculated from the sixth line in each case.

TABLE VI.

A, First Band, First Series			B, Second Band, Second Series		
N, Calculated	N, Observed	Diff.	N, Calculated	N, Observed	Diff.
13168.29	13168.29	0.00	14526.27	14526.27	0.00
168.03	167.81	+0.22	524.84	520.15	+4.69
167.26	166.89	+0.37	520.53	513.49	+7.04
165.98	165.61	+0.37	513.36	506.29	+7.07
164.19	163.94	+0.25	503.33	498.65	+4.68
161.88	161.88	+0.00	490.42	490.42	+0.00
159.06	159.39	−0.33	474.65	481.70	−7.05
155.73	156.51	−0.78	456.00	472.45	−16.45
....	....	......	....	....	......
....	....	......	....	....	......
142.65	145.70	−3.05	424.49	462.72	−38.23
....	....	......	....	....	......
131.37	136.58	−5.21	319.77	418.53	−98.76
124.96	131.43	−6.47	282.92	406.26	−123.34

If the law were accurate, the precision of the measurements would not allow a difference of more than a few hundredths in the last column, whereas they are generally several hundred times as great. The character of the variations plainly indicates that Deslandres' constant b is not really a constant, at least for this spectrum. A glance at the following values of b indicates this clearly. Two series from the B group are selected to illustrate the character and magnitude of the variations in the two band-series.

TABLE VII.

First series, first band, B		Second series, second band, B	
$b = -0.71$	when $n = 1$	$b = -6.120$	when $n = 1$
−0.465	2	−3.195	2
−0.397	3	−2.220	3
−0.366	4	−1.726	4
−0.345	5	−1.434	5
−0.330	6	−1.238	6
−0.321	7	−1.098	7
−0.314	8	−0.993	8
−0.308	9	−0.911	9
		−0.846	10
		−0.792	11
		−0.748	12
		−0.710	13

The variations are not only alike in general in the bands of the same band-series, but the values of b for homologous lines are approximately the same.

If the values of b are plotted as ordinates and the values of n as abscissæ, curves are obtained which at once suggest a much better law.

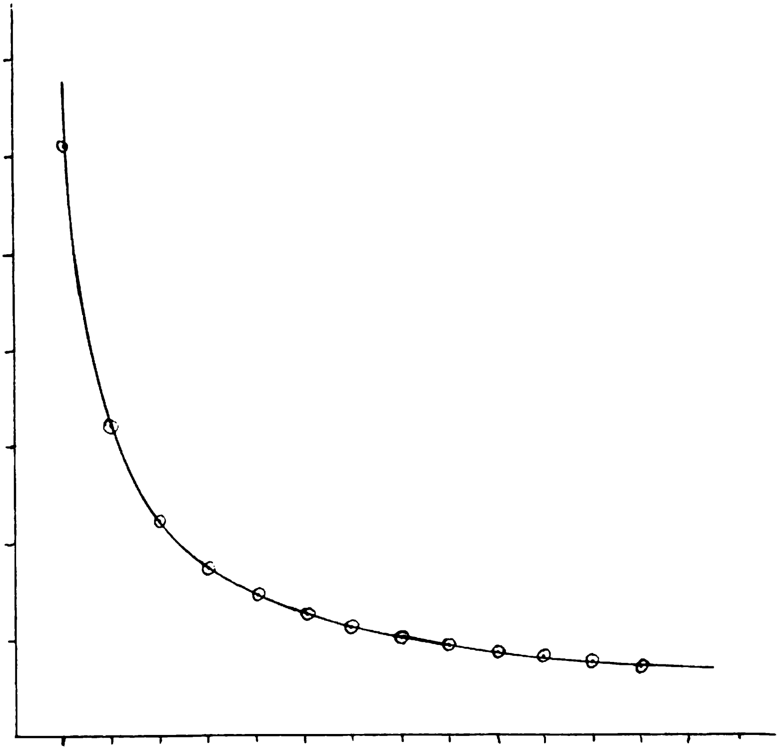


FIG. 2.—The bn-Curve for the Second Series, Second Band.

The two curves shown above are good types of all the bn-curves for the two band-series. Their form is very nearly an equilateral hyperbola, and assuming it to be such, we have

$$bn = k = const.$$

This, however, is not quite true, as k is still subject to a systematic variation which may be corrected as follows:

$$bn - \frac{n}{c} = k.$$

Here k is almost exactly constant, as may be seen by an application to the second series, second band, of B, and to the corresponding series of α.

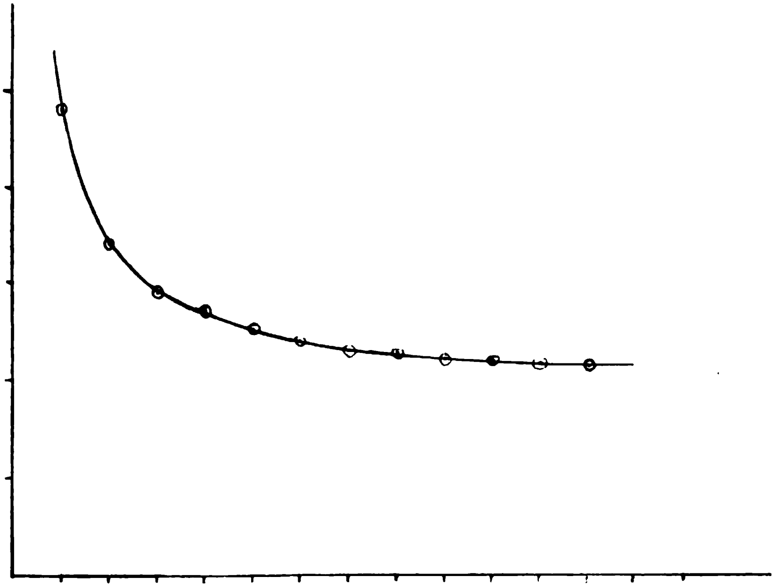


FIG. 3.—The *bn*-Curve for the Second Series, First Band A.

TABLE VIII.

$$bn-\frac{n}{c}=k$$

B	α
5·94	5·94
5·92	5·93
5·90	5·92
5·91	5·93
5·89	5·92
5·92	5·96
5·90	5·94
5·90	5·94
5·90	5·96
5·90	5·97
5·81	5·89

Hence we may write b as

$$b = \frac{k}{n} + c^{-1},$$

and substituting this value in Deslandres' formula, we have

$$N = a + kn + c^{-1}n^2.$$

That is, the correction for Deslandres' law is of the first order in n instead of being as usual of a higher order, the constant k being large in comparison with c. The constants c and k are different for the different series, but their variations are small.

The increased accuracy of the new formula is shown by its application to the same series of A and B calculated before (p. 95) by Deslandres' law. The differences only are given here corresponding to the third columns above. The formulæ used in calculation are as follows: for the A-series,

$$N = 13168.29 - 0.305n - 0.1953n^2;$$

and for the B-series,

$$N = 14526.27 - 5.86n - 0.2611n^2.$$

TABLE IX.

A, First Series, First Band			B, Second Series, Second Band		
Calculated	Observed	Diff.	Calculated	Observed	Diff.
		0.00			0.00
		−0.02			0.00
		+0.01			+0.02
		+0.01			+0.05
		0.00			0.00
		0.00			+0.02
		+0.04			+0.01
		+0.07			+0.01
		+0.04			−0.04
		+0.02			−0.07
		0.00			+0.01
		−0.04			−0.02
		−0.07			−0.05
		−0.11			−0.16

It is quite apparent that the proposed formula is much more accurate than the old one and gives results which agree with observed values to about the degree of precision of the measurements. The smoothness of the bn-curves and the smallness of the differences between the observed and the calculated values of N show that the

lines possess a very regular arrangement; and from the near approach to constancy observed in the second differences of both N and λ it is evident that some law based upon this fact is the true one. Deslandres' formula represents but one out of several possibilities, and, in the case of this spectrum at least, it is not the correct one. It is quite probable also that the proposed formula will be found to fit the line-series of other band spectra more closely than that of Deslandres, but this I have not as yet investigated. If the series were longer, it might be necessary to add another term, possibly one depending in some way on the wave-length. Assuming the law to be exact, we have, on the other hand, a good criterion of the accuracy of the wave-length determinations, and the above table of differences between the observed and calculated values would then show, in general, a greater degree of precision than has been claimed for them.

The new formula may perhaps possess some theoretical importance, inasmuch as in most discussions of a theoretical nature it is tacitly assumed that the first power of n does not occur, and systems are sought which can give rise to vibrations conformable to the formulæ of Balmer, Rydberg, and Kayser and Runge. In a recent contribution to the study of the structure of band spectra from a theoretical standpoint H. Nagaoka[1] discusses the oscillations of a system consisting of a ring of negatively charged electrons surrounding a larger positively electrified mass, and he shows that the vibrations of such a system can represent, qualitatively at least, the arrangement of lines actually observed in band spectra as well as explain other phenomena connected with them. But the formula at which he arrives is that of Kayser and Runge, which certainly would not fit this spectrum with nearly the accuracy of the one proposed. Hence it would seem either that the various band spectra are not capable of being represented accurately by a general formula, or that there is some fault either in the premises or in the logic of the theoretical discussions.

The series in the various groups are different, as they are also in the two bands of the same group. Even the two series of the

[1] "Kinetics of a System of Particles illustrating the Line and the Band Spectrum and the Phenomena of Radioactivity," *Phil. Mag.*, (6) **7**, 445, 1904.

same band are not alike, and consequently, while Deslandres' second law may be regarded as an approximation, it is not, strictly speaking, true. Considering the series in the different groups, we find that the actual distance between homologous lines of corresponding series increases with increasing wave-length, but that the rate of progression decreases; *e. g.*, the average rate of progression in A is about 0.22 of a unit, whereas in a' it is about 0.28; but the distance between any two lines of an A-series is much greater than that between the corresponding lines of a'.

That the two series in any band are not alike is evident from an inspection of the series formed by the widths of successive pairs. In both band-series the pairs are much wider at the beginning of a band, and consequently the first series must have an increasing rate of progression over the second. This difference in width is very evident to the eye when pairs far enough apart are considered. It is difficult to decide whether the variable rate affects one or both series, but the following table indicates that, in general, the first series has a faster rate of progression than the second:

TABLE X.

	A		B		a		a'		a''	
	1st B'nd	2d Band	1st B'nd	2d Band	1st B'nd	2d Band	1st B'nd	2d Band	1st B'nd	2d Band
$\delta\lambda_1$	0.225	0.226	0.248	0.251	0.27	0.27	0.2625	0.2800	....	0.30 (about)
$\delta\lambda_2$	0.2216	0.227	0.245	0.250	0.27	0.268	0.2625	0.2801	....	....

$\delta\lambda_1$ denotes the average rate of progression of the first series.
$\delta\lambda_2$ denotes the average rate of progression of the second series.

Also, of the two band-series the second has the greater rate of progression.

The third law holds more nearly for the second series of bands than for the first, but even in the case of the former there seems to be a steady increase in the second differences from A to a''. There are not enough bands in the series to warrant an attempt at a correction of the formula. The third law holds approximately also for any homologous lines, but the variations from band to band are irregular.

From Figure 1 we should expect that if the upper bands were magnified in some proportion to their wave-length, they could be superposed exactly upon the lower bands. This cannot be done, however, except very roughly. In the first place, the rate of progression of the upper bands, being larger, would be magnified also, and consequently only a limited portion of any two bands could coincide. If two bands have such a relation that one could be superposed upon the other, the ratios of their homologous lines should be constant; and they should be constant, or at least admit of very slight variation, even in the sixth decimal place, as that place affects hundredths in the wave-lengths. In reality, the ratios B/A and B/a for the homologous lines of any two series are not constant even to the fourth place. The ratios in the first band-series of A and B form a steadily increasing series, while similar ratios for the second band-series form a series which decreases regularly to a minimum at the fifth pair and then increases in the same way to a maximum at the end. The ratios between B and a give series which first increase and then decrease, the maximum occurring at the fifth pair, where the minimum occurs in the ratios of A and B. Similar ratios between B and the other bands show peculiarities which add to the complexity of the relations of these groups.

The ratios increase more or less regularly from band to band.

$$\frac{B}{A}=0.903+ \qquad \frac{B}{a'}=1.186+$$

$$\frac{B}{a}=1.094+ \qquad \frac{B}{a''}=1.277+.$$

The question of the extent of the bands of this spectrum and of the occurrence of this particular type of band in other spectra is an interesting one. Liveing and Dewar,[1] in their study of the oxygen absorption spectrum, with the gas confined in high pressure steel tubes, observed bands corresponding to A, B, and a, and in addition a diffuse band with maximum intensity at about λ 5785, which agrees with a', solar, at 5788; also a faint narrow band at about λ 5350, which is approximately the position of a'' (λ 5377). They observed further, a strong band extending from about λ 4795 to λ 4750 and a very faint one at λ 4470. The last two bands could not be found in

[1] *Chemical News*, **58**, 163, 1888.

the solar spectrum because of the many fine lines in that region, although, according to Liveing and Dewar[1], Ångström is supposed to have seen in the solar spectrum at times of intense cold all the bands observed by them except the last. However, if they are present, they cannot belong to the same series as the lower bands.

No investigation yet made in the infra-red region is sufficiently accurate to determine whether other bands of the series exist below A or not. The next band, A′, would fall at λ 8510.5 and A″ at λ 9701.2. It is interesting to note, however, that in Langley's[2] bolometer chart there is a group corresponding to the position of A′ and very similar in general appearance to A, having heavier and wider spaced lines, as we should expect. Not enough detail is given to enable one to decide with certainty. From Figure 4 we may get an idea of the appearance of such a band, did it really exist, as well as of other bands toward the violet. The occurrence of similar bands in the ultra-violet spectrum of water-vapor has already been mentioned, and considering the cause of band spectra, it would seem that there is a tendency to produce this type of band also in those compounds in which oxygen forms so large a part of the molecule or aggregate as to impart its own characteristic vibration to the whole.

The points of chief importance in the foregoing discussion may be summarized as follows:

1. The general accuracy of the determination of the wave-lengths of the groups A, B, and *a* has been greatly increased and the series which compose these bands considerably extended.

2. The band *a′* has been measured and its relation to the other groups studied for the first time, and in addition a new group *a″* has been observed and studied at λ 5377.2.

3. The oxygen absorption spectrum has been shown to consist of two distinct series of bands instead of one, the series of bands occurring in pairs just as do the series of lines in a band.

4. Deslandres' first law is shown to be entirely inadequate to represent the line-series of the several bands, and a modification is proposed which gives results agreeing with the observed values to about the degree of precision of the measurements.

[1] *Loc. cit.* [2] *Ann. Astrophys. Obs. Smithsonian Inst.*, 1, 1900.

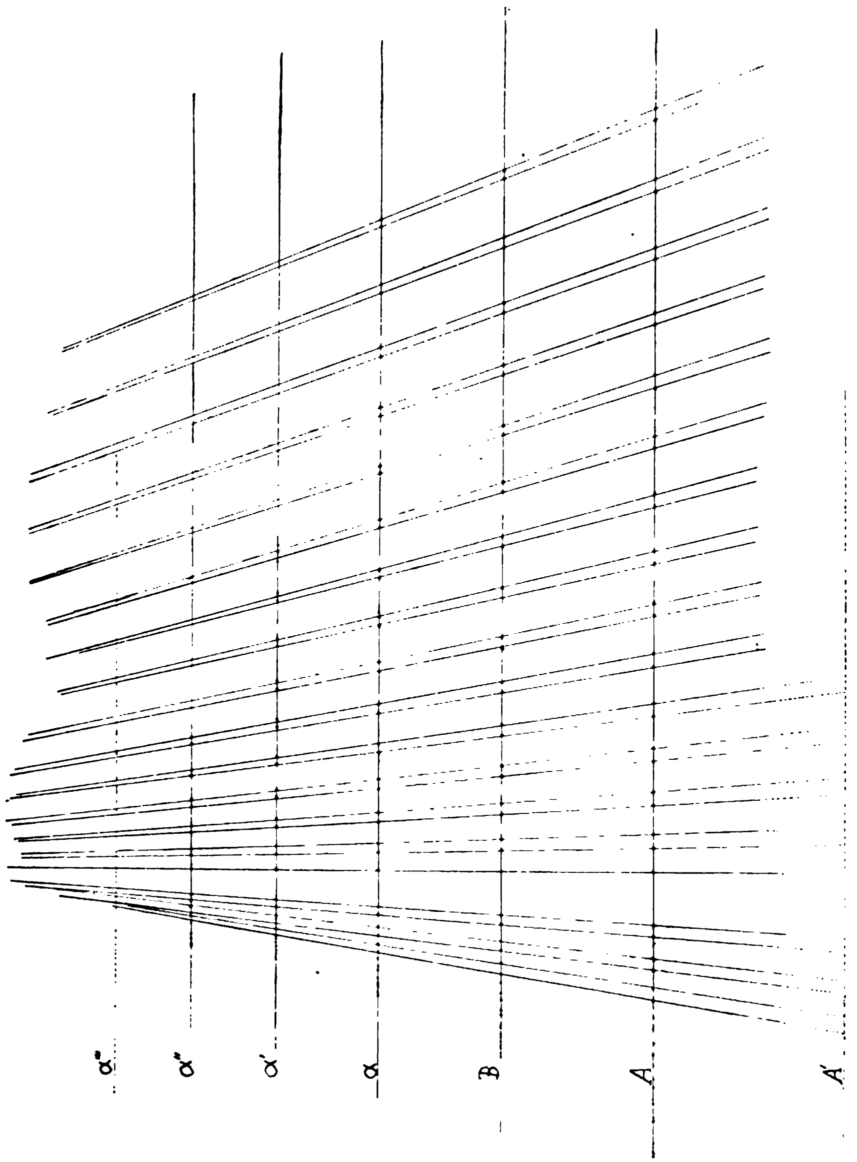


FIG. 4.—The distance between the lines marked A and B is 2 cm, representing the distance in wave-lengths between these two bands. The other horizontal lines are drawn at distances proportional to their distance in wave-lengths from A. Plotted in this way, the homologous lines of the two band-series fall almost exactly on straight lines, this being especially true for those lines whose wave-lengths are most accurately measured.

In conclusion, I wish to express my thanks to Professor A. W. Wright, whose kindly interest and criticism have been of great benefit throughout this investigation, and through whose aid the excellent photographs of the spectrum were obtained.

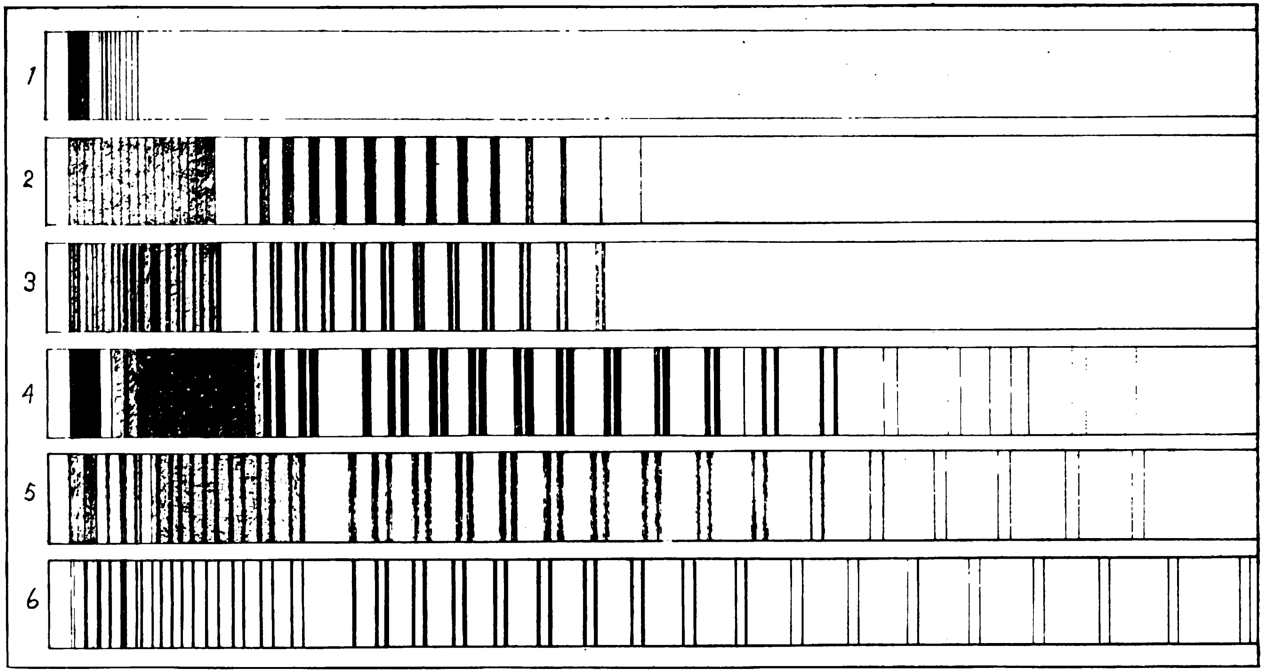
SLOANE PHYSICAL LABORATORY, YALE UNIVERSITY,
July 8, 1904.

EXPLANATION OF PLATE VI.

The drawings in Plate VI show the appearance of the A group as seen by various observers, and also illustrate the progress in the development of more powerful and perfect spectroscopic instruments. The bands are not all drawn to the same scale, which would have been very desirable. It was impossible to do this in some cases. The drawings have all been made for a high Sun, so that the lines appear as sharp as possible.

1. A group; Kirchhoff; prismatic spectrum.
2. A group; Thollon; prismatic spectrum.
3. A group; Langley; grating spectrum.
4. A group; Piazzi Smyth; grating spectrum.
5. A group; Cornu; grating spectrum.
6. A group; drawn from photographs with the Rowland grating in the Sloane Laboratory. The last three pairs are added from measurements of the wave-lengths. The eighteenth pair is cut off. The drawing was finally compared also with Higgs' excellent photographs of this group. All details are shown except the "secondary series."

PLATE VI.



Drawings of the A Group.

THE NEBULÆ IN THE VICINITY OF *NOVA PERSEI.*

By H. SEELIGER.

I HAVE shown in an earlier paper,[1] which I shall designate in what follows by (I), that the phenomena exhibited by the nebulæ in the neighborhood of *Nova Persei*, according to the observations hitherto published, may be completely explained if we assume that they had their origin in the illuminating action by the sudden and brilliant outburst of the *Nova* upon cosmical clouds—this expression being used in its most general sense. Several papers published by others since my former article appeared have led me to return to the subject, as it is my opinion that they are calculated to obscure the substance of the matter, which I believe that I had established.

I shall depart from the same point in this discussion as in (I), but shall merely add a slight generalization. Let the *Nova*, N, be the origin of a rectangular system of co-ordinates, with the X axis extending in the direction toward the Earth, which may be situated at a distance ρ_0, and with the Y and the Z axes in a plane passing through N perpendicular to X. Further, let $r=\sqrt{x^2+y^2+z^2}$, and ρ be the distance of the point (x, y, z), from the Earth. The outburst of the *Nova* will be observed at the Earth later by the time $\frac{\rho_0}{v}$ than it actually occurred, if we denote by v the velocity of light. The time t being reckoned from the instant when the outburst was noted, a radiation proceeding with the velocity v from N will appear to have attained a distance r at the time t, if

$$t+\frac{\rho_0}{v}=\frac{r}{v_1}+\frac{\rho}{v}\ . \tag{1}$$

Accordingly the phenomenon will take the form that would be given if all the points lying on the surface (1) were at the time t encountered by the radiation, be this of any kind whatever. If this radiation consisted of an emission of luminous particles, the particles emitted from N at the time $t=0$ would therefore lie on the surface (1) at the

[1] This JOURNAL, 16, 187–197, 1902; I would remark that certain *typographical* errors in that article were corrected by the editors in Vol. 17, p. 163, 1903.

time t. The eruption of N evidently lasted only for a very short time Δt, of a few days, in such powerful intensity as to have produced appreciable effects at not too small distances r. All the particles which possess or receive an appreciable effect of radiation will accordingly appear to lie between (1) and a surface, whose equation follows from (1), if we replace t by $t+\Delta t$. The brightness of the particles situated within this thin stratum will depend upon r, along each surface (1). If the eruption at N was a phenomenon which could be designated as a "light explosion," then the surface brightness is proportional to r^2; otherwise it either depends upon r in the same manner, or it diminishes still more rapidly with increasing r. But even if we proceed further outward from N in the same direction through the thin stratum, we shall encounter particles of different degrees of brightness. For the explosion doubtless lasted several days, while the effective emission of light from N was very considerable after a sudden increase, and decreased slowly at first and then more rapidly later. It would not be difficult to take this fact into account, but such a complication would nevertheless have no significance in a general consideration intended for the purposes of orientation, since Δt is certainly only a small quantity. Therefore let it be assumed that the particles within our very thin stratum are equally bright in the direction of the radius vector.

The surface (1) is produced by the rotation of a curve of the fourth degree, familiarly known as the Cartesian oval. It would be entirely useless in the case before us to base our considerations upon the rigorous equation (1); indeed, the distance ρ is so great relatively to the dimensions of the nebulæ that we may certainly place $\rho=\rho_0-x$. Thus we get

$$t=\frac{r}{v_1}-\frac{x}{v},$$

or, if we further place $x=-r\cos\phi$,

$$r=\frac{v_1 t}{1+\frac{v_1}{v}\cos\phi}, \tag{2}$$

which is the equation of a conic section with its focus at N, the eccentricity of which is $e=\frac{v_1}{v}$, and its semi-parameter $p=v_1 t$.

For $v_1 > v$ and for a negative t, equation (2) represents the branch of the hyperbola convex toward *N; N* is therefore the focus situated outside of this branch. The larger $-t$ becomes, so much more will this branch of the hyperbola be displaced forward in the direction toward the Earth with increasing p. Nebulous structure must therefore have been visible in the neighborhood of the *Nova* before it flared up, in so far as the conditions were present in the direction toward the Earth for the visibility of the effects of radiation. In any case, we could be concerned only with short intervals, and we cannot invoke the testimony of observations, since the necessary photographs of the neighborhood of the *Nova* having sufficiently long exposure and sufficiently high sensitiveness, shortly *before* it flared up, are not available.

It therefore seems to me a wholly erroneous procedure to exclude the case of $v_1 > v$ as inconsistent with the observations, on the basis of these considerations. But, *per contra*, we shall be obliged to do this for the reason that there are not known in physics either radiations or translatory motions of matter having greater velocities than that of light. We must not in astronomy deal with analogies which, though perhaps thinkable, have no foundation in experience! Furthermore, the case $v_1 > v$ presents in a formal way hardly anything different from the case $v_1 = v$, which I treated solely in (I), and which I also still consider to be entirely sufficient.

The phenomenon is most simply interpreted as the reflection of the light proceeding from *N* by an aggregation of matter in the neighborhood of *N* in a state of the most extremely fine distribution. In truth, this interpretation has only to do with facts of the most trivial character, considered qualitatively, and none of the mysterious assumptions come into question which we are otherwise forced to entertain. I would therefore beg to point out again that I discussed the question of the illumination of cosmical clouds, masses of dust, and the like by neighboring fixed stars long before the appearance of *Nova Persei*, and that I demonstrated the possibility of nebulæ in the neighborhood of fixed stars becoming visible solely by the reflection of the star light. This discussion was published at a time[1]

[1] "Ueber kosmische Staubmassen und das Zodiakallicht," vorgetragen in der Sitzung vom 6 Juli 1901 der K. bayer. Akademie der Wissenschaften.

when it was not possible for me to know in any way that nebulous structure was photographed in the vicinity of *Nova Persei*. This remark seems to me of importance, because it shows that the reflection theory was not made *ad hoc;* but it was, on the contrary, developed from considerations totally independent of the appearance of *Nova Persei*. In that paper I reached the following conclusion, among others, from the computations made (pp. 275, 276). If we assume a star with an apparent magnitude of 10.4 and a parallax of 0".01, then a dust cloud which is of itself dark, and is only illuminated by the star, will exhibit at a distance of *several* seconds an apparent surface brightness $c \cdot 10^{-7}$ times the mean surface brightness of the disk of the full Moon, c being a number which does not differ materially from unity. I then proved further that such surface brightnesses ought to be within the range of visual observation. The objections to the explanation of the events transpiring in the vicinity of *Nova Persei* as a result of reflected light must be regarded as *a priori* not well taken, when we consider that *Nova Persei* was shortly after its appearance some ten thousand times as bright as a star of magnitude 10.4; and when we further consider that plates having long exposures with the exceedingly powerful optical instruments, such as are employed at the Yerkes and Lick Observatories, must permit the detection of far fainter surface brightnesses than that assigned above.

In applying this reasoning to the case before us, it will be a wholly unjustifiable as well as an inadmissible limitation to assume that the cosmical clouds in the neighborhood of N were of homogeneous structure throughout. It would be equally unpermissible, and unfounded either on observed facts or on theoretical considerations, to assume that the radiation of the *Nova* itself should have proceeded with equal intensity toward *all* sides, as I emphatically pointed out in (I), and there considered in detail. In fact, I have no doubt that all the considerations which predicate uniformity for the radiation toward all sides must be dismissed as valueless and inapplicable.

We may now first follow further the assumption that the cosmical clouds surrounding the *Nova* exhibit points of condensation which are arranged according to surfaces or curves. We should then see the cross-section of this structure with the paraboloid (2) pertaining to the appropriate time t. I showed in (I) that for this case: (1)

Every brighter curvilinear region in the visible nebulous structure, as well as its alteration with the time, can be represented within wide limits by the reflection theory; and similarly for the motion of a bright spot. (2) This representation is possible for every assumed value of the parallax, whence it is impossible to determine the value of the parallax by the measurement of the photographic plates without invoking the aid of more or less arbitrary hypotheses. And I also added in (I) (p. 192): "It is hardly necessary to remark that the moving bright spot will retain almost the same form if the mass is distributed homogeneously along the wisp; otherwise its form must change." It would seem to me that this remark had been overlooked or misunderstood by others. The state of things is this: If a powerful outbreak from *N* took place toward a definite direction, and if the matter was pretty uniformly distributed for long distances in this direction, and exhibits no considerable condensations, then the phenomenon will be the same as if a luminous frustum of a cone continued in motion in the original direction with the velocity of light, like a large projectile having a base of any given form. The cone-frustum will change its dimension only in the direction perpendicular to r, and, in fact, proportionally to r^2. For values of r that are not too small, hence at larger distances from *N*, this increase of surface will be slight, and the form of the luminous spot will retain all of its characteristic features. In this respect the state of things is just as if the cone-frustum consisted of projected matter, except for the fact that small irregularities in the density of the cosmical cloud which the cone-frustum encounters permits an explanation of the changes in form and in motion—which is not readily possible in the case of the assumption that we are actually dealing with projected matter.

It has recently been asserted that by adopting what is in a certain sense a photometric point of view we may nevertheless arrive at a determination of the parallax from the measurement of the photographic images of the nebulæ. In consequence of the expansion of the radiation proceeding from *N*, and the attendant weakening of its effect, the nebulous regions can be followed only to a certain ***maximum*** distance r from *N*; and it is taken to be possible to find at every time t the nebulous matter which is situated at this maximum distance r from *N*.

If we disregard the difficulty, not to say the practical impossibility of fixing the apparent distances $\eta = \sqrt{y^2+z^2}$ from N of such nebulous regions corresponding to a maximum value of r, because they must be just at the limit of perception, and if we examine the relationships which will arise, we get the following result:

$$\eta^2 = r^2 - x^2 = r^2 - \left(\frac{v'}{v'_1} r - v't\right)^2 .$$

If it is now assumed that r is the assumed constant maximum distance, then

$$\eta d\eta = \frac{v'^2}{v'_1}(r - v'_1 t) .$$

If we follow such a region along, the velocity $\frac{d\eta}{dt}$ will become constantly smaller with increasing t and η, and we could therefore determine the time t at which $\frac{d\eta}{dt} = 0$. From $r = v_1 t$ we could then find $\eta = v_1 t$, and if we could assign something like plausible values for v_1, the parallax of the *Nova* could be thence determined, as η was measured in seconds of arc.

This train of reasoning, however, makes use of an hypothesis of the class which I have designated as *arbitrary* in (I). Since the radiation proceeding from N can under no circumstances be regarded as equally intense in all directions, as shown above, the maximum r can in no wise be regarded as independent of the time, as the observed η applies at different times to different values of the angle ϕ. The condition $\frac{d\eta}{dt} = 0$ gives rather

$$0 = \left[r\left(1 - \frac{v'^2}{v'^2_1}\right) + \frac{v'^2}{v'_1} t\right]\frac{dr}{dt} + (r - v'_1 t)\frac{v'^2}{v'^2_1} .$$

Here $\frac{dr}{dt}$ is wholly unknown, and the attempt to determine the parallax, even with the assumption for v'_1, is wholly *misdirected*.

In spite of the great simplicity of the reflection theory, many astronomers, for reasons not known to me, have felt themselves called upon to imagine in the flaring up of the nebulæ near *Nova Persei* effects of radiations of other sorts, such as kathode rays, the emission of ions, etc., for which v'_1 would be assumed as materially smaller than v. In (I) I have expressed my view that

this procedure is inadmissible, at least as long as the simple reflection theory has not been completely shipwrecked; that is to say, has come into obvious contradiction with the results of observations, which has thus far by no means occurred.

Nevertheless I should like to touch on one question which concerns the case $v_1 < v$. Curve (2) is then an ellipse with a semi-parameter $p = v_1 t$ and the eccentricity $e = \frac{v_1}{v}$. The major axis of the ellipse lies in the direction toward the Earth and the most remote points from N lie at a distance r_1 from N in the direction toward the Earth, where

$$r_1 = \frac{v_1 t}{1 - \frac{v_1}{v}} .$$

Hence we see that the greatest extent of this ellipse is not very large in comparison with p, if v_1 is materially smaller than v. The radius vector to the extremity of the minor axis makes with the direction of the negative X axis the angle ϕ_1, which is determined by the expression

$$\tan \phi_1 = -\sqrt{\frac{v^2}{v_1^2} - 1} \; ; \quad \text{or } v_1 + v \cos \phi_1 = 0 \;,$$

whence ϕ_1 is independent of the time t. The luminous particles will therefore appear to fill up the space between the ellipsoids which correspond to the two values of the semi-parameter,

$$p = v_1 t \quad \text{and} \quad p = v_1 (t + \Delta t) \; .$$

The assumption of an equal intensity of the radiation in all directions, and of no lack of homogeneity in the cloud structure, now has as its consequence that the luminous nebulæ must appear as an accurate *circular disk* with N in the center. The radius of this circle is the apparent magnitude of the semi-minor axis b. This circular disk will not appear equally bright throughout, but it will, in general, fall off at first as we pass from the center to the edge, since we must anyhow assume that the intensity of the radiation emanating from N will decrease with increasing r. If we make the further, and certainly very plausible, assumption that the luminous nebulosity is so sparsely distributed as never to be wholly opaque, then the apparent surface brightness h at some point at an actual

distance r from N will be proportional to the thickness Δx of the stratum at this point, measured in the direction parallel to the direction toward the Earth. For orienting ourselves, let us assume the stratum to be perfectly transparent, when

$$h = j(r)\Delta x \ ,$$

where $j(r)$ denotes the dependence of the intensity of the radiation upon r. We shall assume that the whole stratum between the two ellipsoids is effective. Then we get by differentiation

$$\Delta x = \frac{vv_1 \Delta t}{v_1 + v \cos \phi} \ .$$

For the extreme edge of the circular disk, however, $v_1 + v \cos \phi = 0$, *i. e.*, $\Delta x = \infty$; whence the edge must appear especially bright. We can of course carry out the computation more accurately and avoid the infinity, as well as to take account of any absorption that may occur; but the result remains the same in sense.

In any case, for smaller values of t, the edge of the circular disk would appear relatively bright; and under some circumstances this brightness would appear so conspicuous that, in addition to the fainter diffuse illumination around the *Nova*, there should appear a brighter circular ring with its center at N which expands proportionally to the time. If the luminous particles do not extend to the extremity of b, the ring disappears, and there remains only the circular disk filled with diffused light. It could hardly be asserted that the nebulæ near the *Nova* exhibited phenomena like these just described. There can be no thought of a radiation from N uniform toward all directions, even if we should wholly disregard an irregularity of the naturally dark masses in the vicinity of N, or of N itself.

With this is excluded also the possibility of determining the parallax, say by measurement of the apparent magnitude of the semiminor axis b, since it cannot be determined whether an appreciable radiation occurred in the direction then coming in question, which is determined by the angle ϕ_1. It could not therefore be asserted that the nebulous regions apparently most remote from N belong to the above mentioned angle ϕ_1, and the determination of the correct value of the angle ϕ_1 cannot be carried out.

MUNICH, June 7, 1904.

THE SILVER "GRAIN" IN PHOTOGRAPHY.

By Robert James Wallace.

ON THE SILVER "GRAIN" IN A DEVELOPED PHOTOGRAPHIC PLATE, WITH A CONSIDERATION OF THE INFLUENCE OF THE DEVELOPING AGENT AS MODIFYING ITS SIZE OR CHARACTER.

It is a matter of importance to those making use of photography as a means of recording scientific data that a definite understanding be arrived at regarding the size of the silver particles which constitute the image. More particularly is this so in the case of astronomical photography, where the original negatives must necessarily undergo considerable enlargement, in order that the detailed structure of the object photographed may be rendered readily apparent.

In many instances this enlargement is carried to such an extreme that the individual particles of silver composing the negative image become so obtrusive that detail is entirely masked (for close observation) and can be discerned only when the enlargement is held off at some distance. In such a case there is absolutely no gain but rather the reverse, as it is much easier to study detailed structure of any kind when at the distance of normal vision.

It is with special reference to this usage that the present work was begun, since there are many plates available for the astronomer or physicist, each of which is supposed to combine in itself (according to the manufacturer) those qualities which make them valuable, viz., speed, fineness of grain, and general uniformity.

Speed must necessarily be the first consideration, and this narrows down the number, so that in the present work those selected for test were as follows: Seed 27, "Gilt Edge;" Cramer "Crown;" Cramer "Instantaneous Isochromatic;" and Hammer "Special Extra-fast." In each instance the plates were taken from different emulsions, triplicate exposures being made for all results aimed for.

These makes were selected as being the fastest generally available, and tests for relative speed showed that the point of highest efficiency was about equally shared by the Seed "27" and the Cramer "Crown." The results of nine separate tests (from different emulsions of the

same trade brand) showed that, although the latter plate was *occasionally* a trifle faster, yet the Seed "27" was always of the same uniform speed, and gave the least amount of "chemical fog." The remaining two plates were somewhat lower in general sensitiveness, which may be proportionately represented as 9:11 between the "isochromatic" and "27", and 8:9 between the Hammer and "27," in favor of the latter.

It is very generally understood that silver bromide ($2AgBr$) is the chief substance employed in the making of gelatine dry plates, but that silver bromide exists in several different allotropic forms has long been known—the first, formed by the admixture of the gelatine and bromide and silver salts, is of low sensitiveness, but in the process of ripening passes gradually through several modifications, finally ending in a state which is capable of reduction by a developer without the previous action of light, viz., the blue allotrope of silver bromide.[1] If the ripening of the emulsion be stopped prior to the formation of this last form, the result is still another allotrope, which is green by transmitted light, and of high sensitiveness.

In the process of "ripening," which is brought about principally by an increase in the temperature of the emulsion, the introduction of ammonia, etc., it undergoes still another and purely physical change, viz., the particles of $2AgBr$ increase in size, in all probability due to accretion. The measurements of Eder give the particles in the finer ***unripened*** emulsion as from 0.0008 to 0.0015 mm, while in the most sensitive form he gives a size of from 0.003 to 0.004 mm.[2] As stated by Perrine,[3] the size in this latter state is approximately 0.0025 mm, while Kayser[4] places it at 0.001 mm.

Although a more sensitive emulsion certainly means a coarser grain, yet that coarseness of grain is not synonomous with speed is shown by the researches of Luppo Cramer, who instances the use of too strong a solution of nitric acid in the manufacture of the emulsion, or too strong ammonia (or too great a quantity), also an insufficient amount of gelatine; even the shaking up of the sediment during the process of "cooking" is likewise certain to result in coarseness.

[1] Blue by transmitted light.

[2] MELDOLA, *Chemistry of Photography*.

[3] *Amer. Annual of Photography*, 1904, p. 203. [4] *Handbuch der Spectroscopie*, I, 638.

Much has at various times been written regarding the value of certain developing agents as modifying the resultant size of these silver particles, and many claims have been advanced by the advocates of slow or dilute development as giving a negative with a very fine grain, and some diversity of opinion exists regarding the plate best suited for the work.

It goes without question that the plate best suited to the needs of the astronomer or physicist is that one which combines the highest speed with the requisite fineness of grain; for in the first case the higher the speed, the greater the efficiency of the telescope or instrument used; and in the second case the finer the grain of the original negative, the greater the available enlargement.

After consideration, the following method was adopted in making the tests as offering the least chance for error (as already mentioned), triplicate plates being made in every case, and at every point throughout the series.

An instrument was constructed upon the same lines as the sensitometer of Scheiner, consisting of a rectangular box open at either end and fitted with forty-two small rectangular cells. One end is closed by a sheet of thin metal in which is made a number of minute holes corresponding to the ends of the small rectangular cells, beginning with one and increasing in arithmetical progression. The plate under test was placed at the other end of the sensitometer and closed in light-tight. Exactly similar exposures were then made to ground glass illuminated by sky light.

COMPARISON OF SIZE OF "GRAIN" IN THE PLATES CONSIDERED.

Similar sensitometer exposures were made on each of the four makes of plates, as previously mentioned, the precaution being taken in the selection of the three plates of each that they should represent entirely different dates of emulsion. These exposures were then developed four at a time, the developing tray containing one plate of each "brand."

A hydrochinone + metol + adurol combination was selected as the developing agent, for reasons which will be obvious as this article progresses. Microscopic and photometric examination of the negatives showed results identical on each plate of the same make.

Photomicrographs were then made from equal opacity squares of one plate of each make, of sufficient magnification to show clearly the individual particles of reduced silver.

Considerable difficulty was experienced in this portion of the work owing to the definite thickness of the gelatine film on the plate and the consequent number of planes, which rendered focusing a matter of extreme care. The use of a lower-power objective could not be taken into consideration because the "grain" was not sufficiently resolved, an entirely false effect being thereby produced.

A magnification of + 430 (Fig. 1) being decided upon, the apparatus was firmly clamped and negatives made of the squares selected.

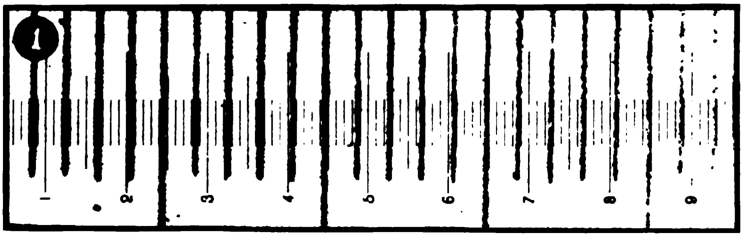


FIG. 1.—Scale of Enlargement for Photomicrographs.
4 divs. of ocular micrometer = 1 div. of stage micrometer;
1 div. of stage micrometer (ruling = 0.001 mm) = 4.3 mm; ∴ 1 div. ocular = 2.5μ.

At this point the triplicate plate exposure was of much assistance, for, as the field was entirely re-focused between each, the plates served as a check upon one another inasmuch as imperfect focusing brought into the field a new "set" of grains whose position would not coincide relatively with the accompanying two plates; to facilitate comparison and eliminate errors of personal measurement, an ocular micrometer was introduced, and brought to a focus at the plane of the sensitive plate.

It will be seen (Figs. 2–5, Plate VII) that the grain particles of the Seed "27" plate are decidedly the most regular of the series, while those of the Cramer "Crown" show the largest and most "ragged." It will also be noted that in the case of the isochromatic plate the appearance of the grain particles is altogether different from that shown by the others; for, while the general shape is more or less regularly round or spherical, the isochromatic grain is decidedly spicular.

PLATE VII.

Seed 27.
Gilt Edge."
1.4 to 1 8μ)

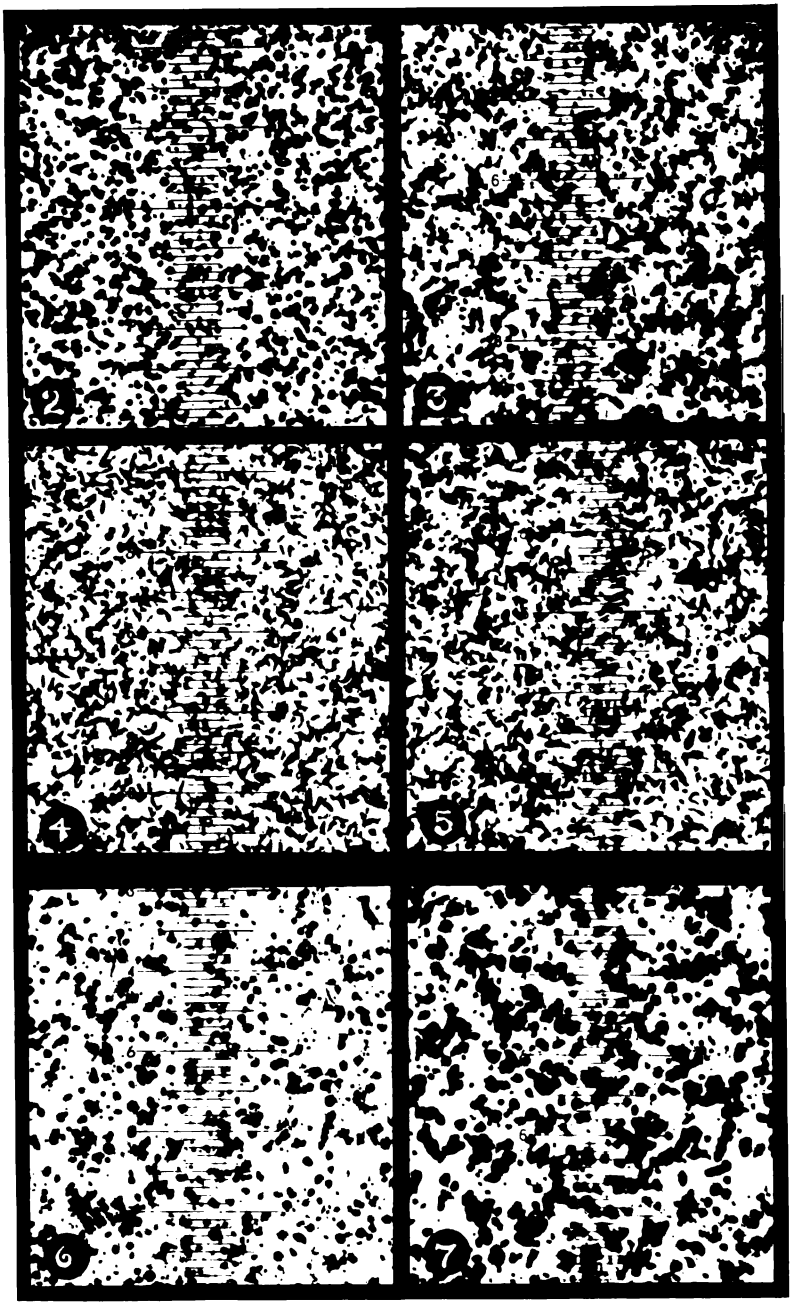
Hammer
"Special Extra
Fast."
(3.5μ)

Cramer
"Inst. Iso."
0.6 X 1.5μ to
1.1 X 3.7μ)

Cramer
"Crown."
(5.3μ)

Seed 27.
Before Intensification.
(1.3 to 4.0μ)

Seed 27.
After Intensification.
(2.8 to 10.4μ)



PHOTOMICROGRAPHS OF SILVER "GRAIN."

Examination shows that this spicular grain is not distributed equally throughout the depth of the film, but is almost entirely confined to the surface; for, whereas the film at this point is composed of grains of this character, they become fewer as the lower planes are successively observed, until at the lowest depth (in contact with the glass) they are entirely absent.

It must not be understood that these spicular "grains" gradually change shape and become irregularly round; they are a distinct set of particles by themselves, the gradual introduction of the others being readily seen with a sufficiently high power.

That this form of particle would seem to occur rather generally in isochromatic plates is shown by the fact that from a number of other "iso" plates, exposed and developed in the same way, and including Cramer "iso" plates of emulsions covering a period of eighteen months, Seed orthochromatic, Cramer trichromatic, and Lumière orthochromatic, the same spicular grain was noticeable in each, but varying in amount. The idea that this form of "grain" is inherent in and essential to an orthochromatic film the writer does not advance, there being several probable reasons for the formation, which it is purposed to treat to a further investigation.

Careful visual microscopic examination of these negatives also agrees well with the findings of Luppo Cramer, who states that each particle of Ag in the negative corresponds to a $2AgBr$ grain in the undeveloped layer, and that an increase in the exposure and development of the plate shows, first, "that the number of Ag grains in the upper layer is constant" (or nearly so); "second, the number of grains in a unit of volume increases; and, third, that the size of the individual grains increases;" and to this may be added a fourth point—that the grain particles become more complex by reason of their running together and forming what we may term as a *group-particle*.

ENLARGEMENT OF THE "GRAIN" BY INTENSIFICATION.

A sensitometer negative was firmly fastened upon the microscope stage and photomicrographs were made. The objective was then swung aside, and the square under magnification was intensified by mercuric chloride, followed by blackening with ammonia, the entire operation being performed by means of a small camel-hair brush.

When dry, the objective was replaced in position; and exposures were again made, which are reproduced in Figs. 6 and 7. The enlargement in size of each of the original particles is very marked, measurement of the "before and after" effect showing an increase from about 2.5μ to 5.0μ in the single grains, and from 4.0μ to 10.5μ in the group-particles. Observation will readily identify the individual grains in these two plates.

INFLUENCE OF DILUTION OR CONCENTRATION OF DEVELOPER, AND TIME OF DEVELOPMENT AS AFFECTING SIZE AND CHARACTER OF "GRAIN."

For this purpose a number of exactly similar exposures were made upon Seed "27" plates in the sensitometer and developed separately at the same temperature by different developing agents as follows:

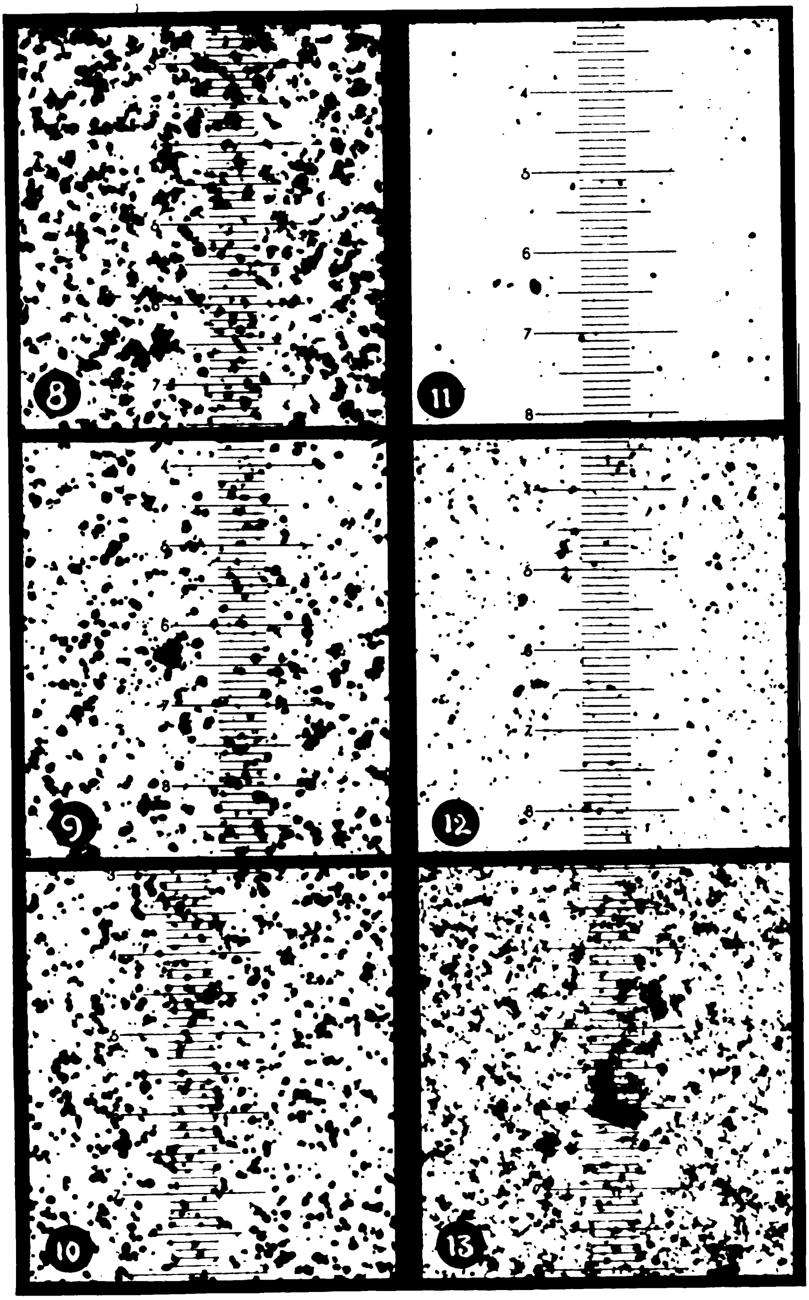
1. Rodinal. Development begun at 1:120 and continued for 15^m; successive additions of rodinal in single minims were made until the developing solution represented a strength of 1:40, taking 35 minutes more time. The 1:40 solution was then allowed to act for 10^m. Total time of development, 1^h.
2. Hydrochinone and caustic potash. Total time of development, 6^m.
3. Hydrochinone + metol + adurol + caustic soda. Total time of development, $1^m\ 20^s$.

These negatives were then dried in a current of air, and an equal opacity square selected from which photomicrographs were made.

A comparison of the results (Figs. 8–10) shows that in the case of the slow development by rodinal the character of the grain is vastly different from that of the remaining two plates, being decidedly more "ragged" in appearance, and showing an actual and definite increase in size, principally by reason of the running together of the several particles to form a new *group-particle*. In the case of the plate developed with hydrochinone, the "grain" is better, with less running together, while in that developed rapidly in the hydro-meto-adurol mixture the grains of silver are seen to be deposited in a much more definite and regular form than in either of the two preceding.

These results in the opinion of the writer, accord well with the theory of increase by accretion, in the length of development; for, according to Abney, when the silver bromide ($2AgBr$) is acted upon by light, there is first a chemical change to Ag_2Br (sub-bromide), followed by a physical change in the $2AgBr$ molecules, and the black-

PLATE VIII.



Seed 27.
Rodinal, Slow
Dev.
(3.0 to 8.7μ)

Seed 23.
Reduction
without ex-
posure.
Dev. for 45
secs.

Seed 27.
Hydrochinon,
Medium Dev.
(1.3 to 2.4μ)

Seed 23.
Reduction
without ex-
posure.
Dev. for 10 min.

Seed 27.
Hydro-Metol-
Adurol, Rapid
Dev.
(1.4 to 1.8μ)

Seed 23.
Reduction
without ex-
posure.
Dev. for 20 min.

PHOTOMICROGRAPHS OF SILVER "GRAIN."

ening consists, first, in a slight reduction of Ag_2 and continues by an interaction of the molecules in which the Ag_2 seizes upon the bromine in the adjoining molecule of $2AgBr$, and reduces that to a state permitting of reduction by the developer, while it in turn attracts the bromine of its neighboring molecule, and so on. Second, according to Luppo Cramer, the Ag ions give up their positive charge (caused by the impact of light) to the negative ions in the developer (which arise from the soluble salts in the developer), and the resulting supersaturated solution of metallic silver is deposited upon the nuclei of the Ag in the film; therefore, whatever theory be preferred, it naturally follows that the longer the action of development is continued, the greater will be the size of the particle, either by interaction or deposition.

The increase in the size of the individual grains was also very readily seen by developing a stained plate under the microscope. A drop of developer was applied while the $2AgBr$ grains were under observation, and an almost immediate reduction was observed. By repeating the experiment a number of times it was seen that the grains of silver bromide, when first acted upon by the developer, were reduced as individual grains of very small dimensions[1], only a portion of the $2AgBr$ particle appearing to be acted upon at first, but increasing in size individually and then coalescing into group-particles, which become larger as development is continued.

REDUCTION OF THE SILVER BROMIDE WITHOUT PREVIOUS EXPOSURE.

That there is a definite reduction of the $2AgBr$ particles in the film even when there has been no light impact, is well known, although not generally understood. The appearance of "chemical fog" upon the film is used by many workers as an indication of the point of maximum development.

That this "chemical fog" begins to be deposited almost from the instant of contact with the developing agent, was shown by a series of experiments as follows: Strips were cut (in darkness) from

[1]This is in agreement with LIESEGANG (*Archiv. für Wissensch. Photo.*, I, 220), who says: "Since the advance of the reduction on to the unaltered silver bromide proceeds comparatively slowly, we must conclude that it is not always necessary for the groups of particles of silver bromide to be completely reduced. In this way a diminution in the size of the grain may therefore be possible."

a number of plates, including Cramer "Crown," Seed "27," Seed "23," and Carbutt "Lantern slide," and were then partially immersed in fresh hydrochinone developer for different lengths of time, from 5 seconds to 20 minutes, and passed immediately into the fixing bath. When dry, examination showed that reduction had taken place on all plates in amount varying according to the time of development. This reduction was relatively stronger on the "Crown" plate and weakest on the "lantern slide," the Seed "27" and "23" occupying places intermediate. The gain (by accretion) in the size of the grain particles is also very well shown in this series of plates. Figs. 11–13 (Plate VIII) give the record of the Seed "23" plate at 45 seconds, 10 minutes, and 20 minutes development, respectively. The exceeding fineness of the grain-particles in these plates will be noted, the size varying from 0.0017 to 0.002 mm mean diameter.

This reduction of the unexposed silver bromide was found to take place with all the developing agents tested, which include hydrochinone, hydro-eikonogen, metol, rodinal, adurol, and edinol. It would therefore appear that there is present in the film a certain amount of that allotropic form of $2AgBr$ which is capable of reduction without previous light-action, and which amount increases with the sensitiveness of the plate.

The idea generally accepted by a great number of workers that the size of the silver "grain" in a negative is dependent upon the size of the grains of silver bromide originally in the film; or, by still another class, that the slow or dilute development of the plate would give a negative whose "grain" would be distinguished by a particular degree of fineness, is not borne out by the results obtained and herein specified. On the contrary, we are led to deduce from these experiments:

1. That the original grain-particles of the silver bromide are by prolonged development considerably enlarged, by reason of the formation of *group-particles*, which are relatively enormously increased in size, so that a method of *rapid* development (provided the developer is compounded to give not too great a contrast) is the means of obtaining a more definitely uniform deposit of particles, which most nearly approach the size of those in the original $2AgBr$.

2. That of high speed American plates the Seed "27 Gilt edge"

PLATE IX.

LUNAR REGION NEAR GASSENDI.

Negative made with 12-inch visual telescope and color-screen. Diameter of original image, 2 inches: enlarged 11.3 times.

is, of the four makes tested, that having the finest grain-particles, of most definite uniformity; of equal speed with the "Crown," but with less tendency to "chemical fog."

3. That the intensification of the original negative should not be attempted where enlargement is to follow.

As an example of the efficiency of rapid development, attention is directed to Plate IX, which is reproduced from an enlargement of 11.3 diameters. The original negative was taken through a color-screen on a Cramer isochromatic plate; time of development, 70 seconds.

In conclusion the writer takes pleasure in acknowledging the kind assistance of Messrs. E. B. Frost and J. A. Parkhurst in much of the foregoing work, and also takes this opportunity to sincerely thank those who so courteously replied to his communication of the spring of 1903.

YERKES OBSERVATORY,
July 4, 1904.

NOTE.

The foregoing work was begun about the beginning of 1903, at which time the author sent out a circular letter to the departments of physics and of astronomy at a number of the leading universities and observatories throughout the United States, requesting data relative to the plates and developers in general use by them. The pressure of other duties prevented the completion of the work until July of the present year.

Upon completion of the paper, and after arrangements for publication, there came to the notice of the writer an article published by Messrs. A. and L. Lumière and A. Seyewetz entitled "The Influence of the Character of Developers on the Size of Grain of Reduced Silver" The method pursued and therein outlined by these eminent investigators, was as follows: Exactly similar exposures were made upon Lumière "blue label" plates of same emulsion, which were then developed by all the principal known developers (prepared normally and also with modifications) "until the images had reached a comparable density." The portion showing the greatest opacity was then selected from each negative and by the aid of hot water the gelatine was dissolved off. "This gelatine solution, well shaken, and

containing the reduced silver, was used for the preparation of material for microscopic examination." Photomicrographs were then made of the same magnification, and prints therefrom compared.

Among other conclusions thus derived, the investigators state:

"2. No apparent influence is shown in the size of the grain of reduced silver by temperature, concentration, or duration of development."

An evident discordance between this conclusion and that of the present writer, calls for a word of explanation. This apparent discordance would perhaps be easiest resolved if it were decided just what is to be regarded as the "grain" of the negative. In the opinion of the writer the general description "grain" is taken to mean the particles of silver reduced in the negative and *in situ.* It has been shown that the grain-particles of the $2AgBr$ are more or less modified in character, both by the method and duration of development, and the tendency of the grains to coalesce and form *group-particles.* A *group-particle* is undoubtedly formed of individual grains, but inasmuch as their units are now to be taken collectively as a new whole, they must be so considered. Granted even that the size of the individual grains remains unaltered by variation in development; yet, if a number of these particles be so welded together (as it were) by chemical or electrolytic action their *character* would be altered and hence necessitate a new consideration of their gross size.

R. J. W.

PLATE X.

S

STAR-CLUSTER *N. G. C.* 663.

Photographed with 40-inch telescope without color-screen. Enlarged 1.7 times from original negative.
Scale: 1 mm = 6".1.

ON THE STELLAR PARALLAX PLATES TAKEN WITH THE YERKES TELESCOPE.

By FRANK SCHLESINGER.

THE success which attended Professor Ritchey's experiments in photography with the forty-inch Yerkes telescope, and the fine results obtained by him, have led to an attempt by the writer to use the telescope for the photographic determination of star positions, and especially for the measurement of stellar parallaxes. For such purposes the great focal length of the telescope, and the correspondingly large scale of the photographs, would seem to offer an unusual opportunity for a high degree of precision. Professor Ritchey's method was, in brief, to place a yellow color-screen immediately in front of a Cramer isochromatic plate, and to keep the latter stationary (as referred to the image made by the objective) by means of the late Dr. Common's double-slide plate-holder. This simple device of Common's not only makes an auxiliary telescope unnecessary, but insures guiding which is far superior to anything that could be done, in this case at least, by moving the entire telescope.

The screens employed by Professor Ritchey consisted of two approximately flat plates of clear glass inclosing a film of collodion and another of Canada balsam. Such a screen must introduce distortions other than those which are strictly geometrical. Whether these are great enough to interfere seriously with the delicate requirements of stellar parallax work is an open question. Although this disadvantage of the screen is probably not an insuperable one, it was thought better to avoid the difficulty rather than to devise methods for overcoming it. A little experimenting showed that for present purposes the screen can be dispensed with: that is, Cramer isochromatic plates at the visual focus give very good star images without anything between the objective and the plate. The accompanying illustration, Plate X, is from a negative of a loose cluster, *N. G. C.* 663, taken without a screen on October 11, 1903. The length of exposure was one hour, and the atmospheric conditions were good throughout. This negative was taken to show the fainter stars, and

consequently the half dozen bright stars are overexposed. A careful comparison of stellar plates taken with the screen and without shows that there is little to choose between them either as regards the minuteness of the images or their sharpness. This statement, which may seem surprising at first sight, is explained by reference to the accompanying diagram, showing the color-curve of the forty-inch objective and the curve of sensitiveness of the plate for different

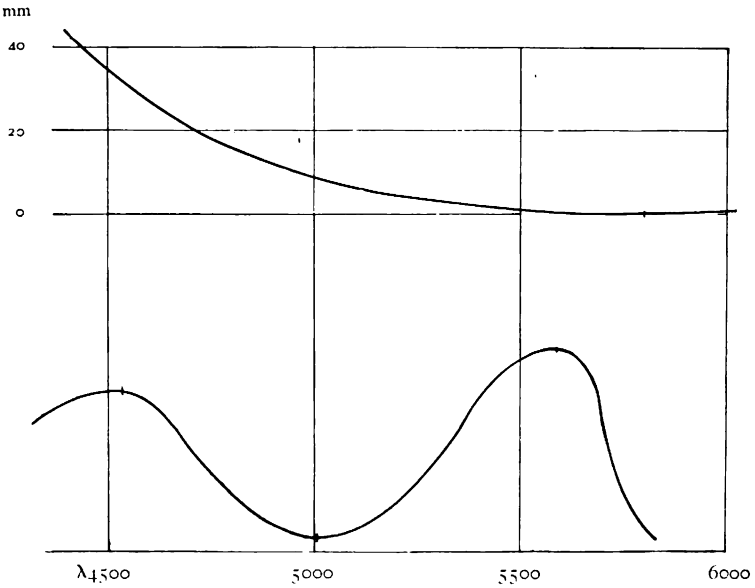


FIG. 1.—Color-Curve of the 40-inch Objective Compared with Curve of Color-Sensitiveness of Cramer Isochromatic Plates.

wave-lengths. The former is copied from page 94, Vol. X of this JOURNAL. The curve for the plates was drawn from a study of a series of solar-spectrum negatives taken by Mr. R. J. Wallace, photographer at the Yerkes Observatory, and kindly loaned to the writer for this purpose. It will be seen that the plates have a maximum sensitiveness in the yellow, at about λ 5600, which falls off very rapidly toward the red end, so that a prolonged exposure is necessary to show the D lines at λ 5890. There is a secondary maximum in the blue at about λ 4550, where the plate is nearly as sensitive as in the yellow. Between these two maxima, that is, in the green, the plates are only slightly sensitive, reaching a minimum at about λ 5000. A

comparison with the color-curve of the objective shows that the only light which is effective in making the stellar images is included between λ 5200 and λ 5700. For, in the first place, the region between λ 4800 and λ 5200 is cut out by the plate itself; and again, the rays beyond λ 4800, toward the violet end of the spectrum, are so much out of focus that the light is enfeebled when it reaches the plate, because it is spread over a circle of considerable area. For example, light at λ 4500, to which the plate is sensitive, is 36 mm out of focus, and therefore reaches the plate as a circle about 2 mm broad in addition to the small diffraction disk. This spreading out prevents the blue rays from affecting the plate unless the exposure is much prolonged. It hardly seems necessary to remark that a screen which excludes blue rays is indispensable if the object to be photographed is a surface (such as the Moon, a nebula, or a very dense star cluster), for in this case the blue rays from different points in the source will overlap to such an extent as to make them as strong, at any one point of the photographic film, as though they were in focus at that point. On the other hand, the best screen for stellar work would not be one which cut out only the blue rays, but rather one which prevented the region λ 5200 to λ 5400 from reaching the plate. However, the improvement effected by such a screen as the latter would probably be slight and noticeable only under the best atmospheric conditions. For stellar-parallax measurements this possible improvement would not compensate for the presence of the screen. Consequently all the plates thus far secured for this work have been taken without interposing anything between the plate and the objective.

The following table shows the degree of accuracy with which the images can be bisected. The first star is the faintest that can be measured on a plate that has been exposed for five minutes under the best atmospheric conditions; the second is about the best size for bisection; and the third is nearly, but not quite, the largest image that can be bisected with confidence.

Magnitude	Diameter	Probable Error of one Setting
10.5	0.07 mm = 0″.75	0.0017 mm = 0″.018
9.2	0.20 2.1	.0012 .013
8.0	0.38 4.0	.0018 .019

It is interesting to compare these with the corresponding figures for the *Astrographic Catalogue* plates. The smallest images on these are perhaps half the size of the smallest on our plates; but the ***angular*** diameter of their images is nearly three times as large, and the probable error in arc of a single setting is three or four times greater than on the forty-inch plates.

The simple error of setting upon an image is well understood to form only a part of the total error in a photographic star place. A real test of accuracy is the agreement between different plates. Below are given four sets of residuals, which are fair samples of the accordance that we usually get. The initials refer to the two observers, Miss Louise Ware (L. W.) and the writer (F. S.). The brackets on the left indicate that the exposures inclosed by them are on the same plate. These residuals are derived from measurements of the double star Σ 2398; they are cleared of the effects of parallax and proper motion as well as refraction, aberration, etc.

From all these residuals we compute the probable error of one exposure to be $\pm 0\rlap{.}''030$.

This agreement among different plates must be considered very satisfactory; it shows that only a few exposures will be necessary to reduce the accidental error in the computed parallax to a negligible quantity. There still remains, however, the more serious question of systematic errors. Previous experience in photographic as well as other work has shown that such errors are especially liable to creep into results, if the observations are made at large hour-angles. Consequently the present series of plates has been restricted to within two hours of the meridian. This can be done only at a sacrifice of "parallax factors;" that is, many of the plates (especially during the summer) are exposed when the effect of parallax upon the star's position is far from the maximum. This sacrifice is made possible by the minuteness of the accidental errors, which compensates in a large degree for the small coefficients. Another precaution which has been taken is to use the telescope only on one side of its pier. This entails an additional diminution of the parallax factors, but it is worth while; for it eliminates a possible distortion due to the objective, as the latter is always presented to the sky in the same position. Reversing the telescope is equivalent to rotating the objective 180°

RESIDUALS FOR THE DOUBLE STAR Σ 2398.

Brighter Star (8.5 Mag.)		Fainter Star (9.3 Mag.)	
F. S.	L. W.	F. S.	L. W.
−0.″080	−0.″053	−0.″059	−0.″035
− .021	+ .011	− .040	− .045
− .056	− .011	+ .013	− .005
− .059	− .061	− .011	+ .056
+ .069	+ .008	+ .053	+ .059
+ .013	+ .021	+ .035	+ .069
+ .037	+ .016	+ .037	+ .040
+ .043	+ .077	+ .008	+ .053
+ .021	− .013	+ .013	− .003
+ .032	+ .024	+ .024	+ .019
+ .013	− .027	− .035	− .045
− .027	− .061	− .035	− .043
+ .021	+ .045	+ .008	+ .027
+ .027	+ .013	+ .011	+ .053
− .045	− .077	+ .029	+ .048
+ .027	− .016	+ .048	+ .029
− .019	− .043	− .037	+ .029
− .059	− .051	− .016	+ .019
+ .056	− .005	− .008	− .043
+ .056	+ .080	+ .021	+ .037
− .051	+ .053	− .061	.000
+ .051	+ .008	+ .019	+ .035
+ .029	+ .128	+ .037	+ .053
+ .072	− .016	+ .003	− .003
− .035	− .093	− .003	− .064
− .053	− .045	− .027	− .011
− .003	− .043	+ .072	+ .024
− .048	− .059	+ .011	− .008
+ .053	− .016	+ .043	+ .051

around its optical axis. The great weight and diameter of the lenses, and the consequent sag when the telescope is pointed near the zenith, make this precaution especially important with the forty-inch telescope. The phenomenon known as "atmospheric dispersion," to which Dr. Rambaut has called attention,[1] can have no great effect on our plates; not only on account of the small hour-angles at which

[1] *Monthly Notices of the R. A. S.*, **55**, 123, 1895.

the exposures are made, but also because of the limited region of the spectrum (λ 5200 to λ 5700) which is here concerned in forming the image, whatever the color of the star may be.

These precautions, and some less important ones that need not be described here, seem to have been successful in keeping out systematic errors, so far as may be judged from the data thus far accumulated. Some examples of the parallaxes computed from the plates are given below, but it must be borne in mind that these results are only preliminary. Definitive corrections and additional plates will no doubt modify them to some extent.

Krueger 60 (R. A. $= 22^h\ 24^m$, Decl. $= 57°\ 10'$).

This star was put upon the list at the suggestion of Professor Barnard, who inferred (correctly it appears) that the star has a large parallax.[1] One of the stars of *Krueger* 60 is itself a binary in comparatively rapid orbital motion, the components now being about 3″ apart and of magnitudes 9.1 and 11.0 respectively; the system has an annual proper motion of nearly a second of arc. Our plates furnish the following values of the parallax for the brighter component; these are independent of each other, except that Miss Ware's measurements and the writer's refer to the same plates, eight in number, containing twenty exposures.

Parallax	Weight	
+0.268	3	by L. W. from R. A., 5 distant comparison stars.
+0.277	3	by F. S. from R. A., 5 distant comparison stars.
+0.226	1	by L. W. from R. A., 1 close comparison star.
+0.258	1	by F. S. from R. A., 1 close comparison star.
+0.292	1	by L. W. from Decl., 5 comparison stars.
+0.301	1	by F. S. from Decl., 5 comparison stars.

Means, R. A.	+0″265,	weight 8
Decl.	+0.296,	weight 2
Means, L. W.	+0.264,	weight 5
F. S.	+0.278,	weight 5
Means, 5 comparison stars	+0.278,	weight 8
1 comparison star	+0.242,	weight 2

This is the first determination of the parallax of this star; if it

[1] *Astronomical Journal*, 23, 169, 1903.

should be confirmed by other measurements, this faint star would appear to be one of the Sun's nearest neighbors.

Fedorenko 1457, 8 (R. A. = 9^h 7^m, Decl. = 53° 7′).

This is a wide double, each component being of about the eighth magnitude. Values of the parallax of both stars were deduced from only four plates containing eleven exposures. These are not sufficient to determine the proper motion, which is, however, known well enough for present purposes. In this case we have the equation $\Delta\pi = 0.21\ \Delta\mu$, which shows the dependence of the computed parallax upon the assumed proper motion; that is, any error in the latter will affect the parallax by about one fifth this error.

Parallax		
+0.231	Preceding star, measures by F. S.	
+0.231	Preceding star, measures by L. W.	
+0.205	Following star, measures by F. S.	
+0.226	Following star, measures by L. W.	
Means,	F. S.	+0″.218
	L. W.	+0.228
Means,	Preceding star	+0.231
	Following star	+0.216

Peter obtained +0″.18 for the parallax of this star by means of the heliometer.

Struve 2398 (R. A. = 18^h 41^m, Decl. = 59° 29′).

This double now has a separation of about 17″. The components are of magnitude 8.5 and 9.3 respectively, and both could be measured upon our plates. The system is in rapid motion as a whole, 2″.3 per annum. A comparison between the photographic measures and earlier micrometer work upon these stars shows that there is considerable orbital motion, the relative directions having changed nearly 90° since Struve's observations. This motion is somewhat surprising in view of the wide separation and the faintness of the components, and makes the system nearly unique. The values of the parallax given below rest upon eleven plates containing twenty-nine exposures. As in the previous case, the plates are not yet sufficient to determine the proper motion, which has to be assumed for the present. The orbital motion was taken into account in this connection. We have for both stars $\Delta\pi = 0.40\ \Delta\mu$.

Parallax	
+0.287	by F. S. from the R. A., for the brighter star.
+0.299	by L. W. from the R. A., for the brighter star.
+0.283	by F. S. from the R. A., for the companion.
+0.292	by L. W. from the R. A., for the companion.

Means,	Bright star	+0".293
	Companion	+0.288
Means,	L. W.	+0.296
	F. S.	+0.285

Flint obtained +0".32 for this parallax, using Kapteyn's meridian-circle method; and Lamp, by measuring differences of declination with a micrometer, made it 0".35.

YERKES OBSERVATORY,
August 5, 1904.

ON THE TRANSITION FROM PRIMARY TO SECONDARY SPECTRA.

By P. G. Nutting.

The work here described was undertaken to determine as definitely as possible the conditions under which secondary spectra are produced, and to study the effect of slightly varying these conditions in the critical state when both spectra are present. Plücker and Hittorf[1] in 1865 proved that several of the elementary gases may emit two entirely different spectra. With a large capacity in parallel with the tube of conducting gas, they obtained a disruptive discharge which emitted a bright line spectrum, called by them the secondary spectrum. I have undertaken to determine how much capacity was necessary to just produce the secondary spectrum in different gases, and how this critical capacity varies with the wave-length, with the density of the gas, the amount of inductance and resistance in circuit, distance apart of electrodes, and sectional area of the discharge.

Photographic methods were employed throughout. Spectra obtained under varied conditions were photographed side by side on the same plate, so that the minutest changes could be observed and followed. For this purpose a large model Fuess quartz spectrograph was used. This was provided with a large flint glass prism giving a spectrum about 15 cm long from 300 to 600 $\mu\mu$. Ten spectra could be recorded on the same plate. A large glass condenser was used, composed of twenty plates well separated and provided with mercury cups so that the capacity could be varied by a plate at a time. Current was supplied by transformers of 1,000, 2,000, and 5,000 volts, and by a set of generators giving 5,000 volts continuous current. For inductance, a Seibt tuning solenoid of 120 20 cm turns was used. The greater part of the work was done with ordinary short stout Plücker tubes made by Boehm, of Chicago. These had electrodes about 4 cm apart and capillary portions 2 mm in diameter and 12 mm

[1] *Phil. Trans.*, **155**, 1–29, 1865.

long. To keep the pressure of the inclosed gas more nearly constant, a half-liter bulb was sealed to each tube while in use.

With a tube of air at 13 mm pressure, spectra were photographed with capacities of 0.12, 0.09, 0.06, 0.03, and 0.0 microfarad in parallel. A sudden change from secondary to primary spectrum was found to occur at a capacity of 0.04 mf, a capacity equivalent to that of about fourteen one gallon Leyden jars. Adding capacity above 0.06 mf produced little if any effect, nor do secondary lines appear in the primary spectrum until the capacity is nearly 0.03 mf.

Critical capacity and wave-length.—Drawing a line separating primary and secondary spectra on the photographic plate (see Plate XI), the ordinates of the curve represent roughly critical capacity, the abscissas wave-length. The curve drops off very steeply toward short wave-lengths, and in spite of the greater dispersion, indicating that for waves perhaps not shorter than 300 $\mu\mu$ the critical capacity becomes infinite. Critical capacity expressed as a function of wave-length appears to be of the form

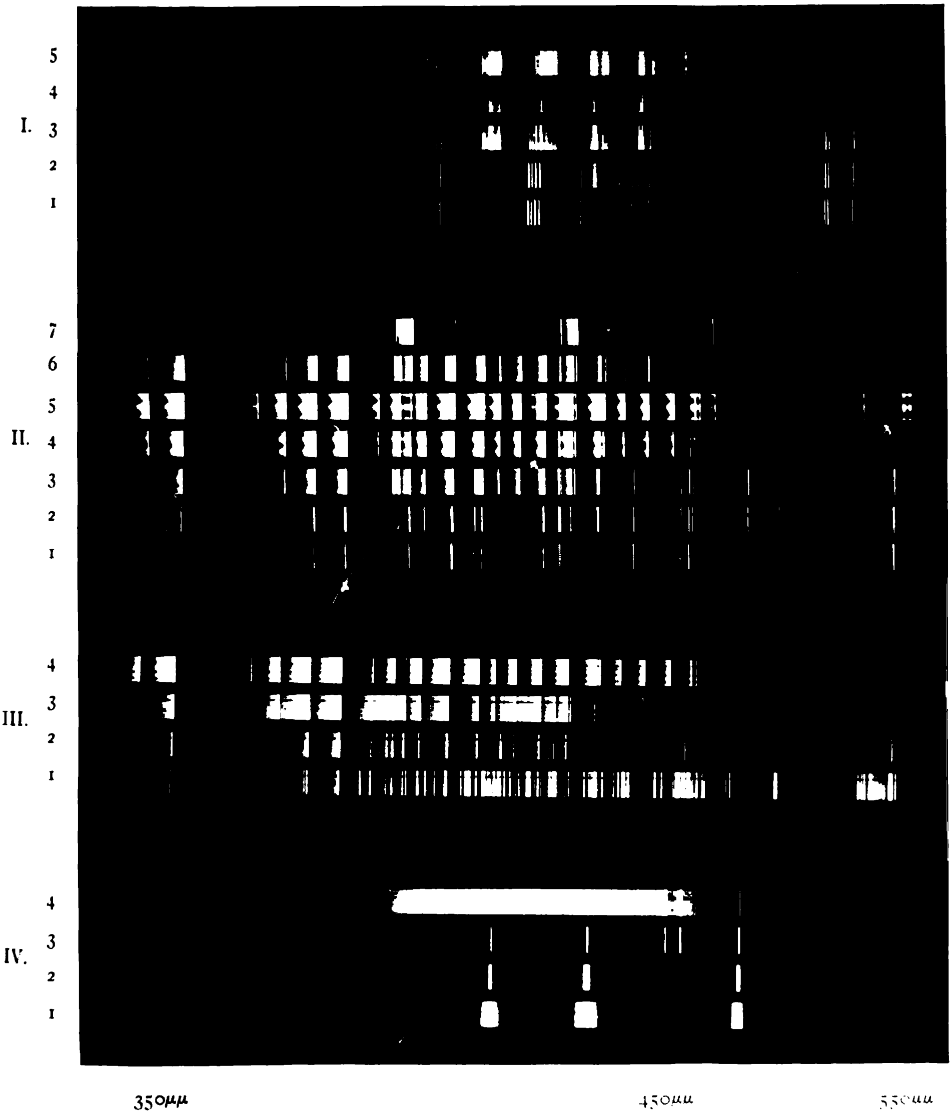
$$C = ae^{-b\,(\lambda - \lambda_0)}$$

Approximate numerical results for air are given in the table:

Pressure	Wave-Length in $\mu\mu$		
	350	450	550
20 mm........	0.07	0.03	0.02 mf.
1 mm........	0.15	0.06	0.03 mf.

Critical capacity and pressure.—The critical capacity increases slightly as the pressure decreases down to about 1 mm air pressure, when it suddenly becomes infinite; *i. e.*, no amount of capacity will cause the secondary spectrum to appear unless an external spark-gap is used. This critical minimum pressure at which the disruptive discharge becomes possible is considerably lower for hydrogen (about 0.1 mm), but depends on the condition of the surface of the electrodes and the presence of impurities. At high pressures the critical capacity continues to gradually decrease. Using a special tube, the distance apart of whose electrodes could be varied to suit the potential (5,000 volts) employed, the critical capacity of air and hydrogen was observed up to atmospheric pressure, when it was found to be about one-fourth

PLATE XI.



I. Sulphur: 1, secondary; 5, primary; 2, 3, 4, combinations of primary and secondary showing their co-existence. II. Nitrogen at 1 mm pressure: 1, taken with 0.12 mf capacity: 2, with 0.09 mf; 3, with 0.06 mf; 4, with 0.03 mf; 5, primary (anode), no capacity; 6, same as 3, but taken in bulb instead of capillary; 7, N cathode glow. III. Inductance effect in nitrogen at 21 mm pressure: 1, 0.04 mf capacity, no inductance; 2, same capacity, 0.12 m. h. inductance; 3, 0.9 m. h. inductance; 4, N primary, neither capacity nor inductance. IV. Hydrogen under same conditions as the Nitrogen in III.

what it was at 10 mm pressure. This would make the critical capacity roughly proportional to the cube root of the pressure, or inversely proportional to the mean distance apart of the molecules.

Critical capacity and nature of the gas.—Hydrogen, sulphur, nitrogen, oxygen, bromine, and iodine were tested and the critical capacity found to be practically the same for all for the same pressure and wave-length. Critical capacity is more sharply marked in sulphur, nitrogen, and iodine. With hydrogen, the lines of the secondary (four-line) spectrum invariably appear in the primary (many-line) spectrum, the capacity of the wires leading to the tube being a considerable factor in this dominance. All the substances show the same great increase in critical capacity for the very short wave-lengths and decrease with increasing pressure..

Critical capacity appears to be nearly or quite independent of the voltage employed (up to 5,000 volts) and of the separation of the electrodes. Tests were made with the electrodes but 3 mm apart. At much shorter distances the metallic lines from the electrodes become prominent at higher pressures.

In the capillary of a Plücker tube the critical capacity is less than in the bulb, or less than in a tube without a central constriction. This is confirmatory to a view expressed in a previous paper,[1] that the production of a secondary spectrum was not so much the effect of capacity *per se* as of increased current density.

Critical capacity and inductance.—The effect of introducing inductance is always to relatively weaken the secondary and enhance the primary spectrum. But putting in a certain inductance is by no means equivalent in its effect to taking out a definite corresponding capacity. Inductance was added in steps of 0.008 millihenry. The first inductance added, though very small, weakened the secondary spectrum very markedly and introduced primary lines, and this whether the capacity used was just above the critical capacity or five times that amount. Adding more and more inductance produces less and less additional effect. Apparently no amount of inductance, however great, will *completely* annul the effect of any capacity, however small. Capacity and inductance effects are shown graphically in the figure. Ordinates represent the change from primary to secondary spectra.

[1] ASTROPHYSICAL JOURNAL, May 1904.

Critical capacity and resistance.—The effect of resistance is as pronounced as that of inductance in changing the secondary spectrum back to the primary. Even as little as 20 ohms (non-inductive) resistance brings in primary lines, while 100 ohms gives a nearly pure primary. The resistance effect curve has very nearly the same form as the inductance effect curve, as shown in the figure.

Critical capacity of mixtures.—Mixtures of hydrogen and nitrogen, sulphur and hydrogen, iodine and nitrogen, iodine and hydrogen, nitrogen and sulphur, hydrogen and oxygen, and mercury and nitrogen were tested, and each component was found to have its own

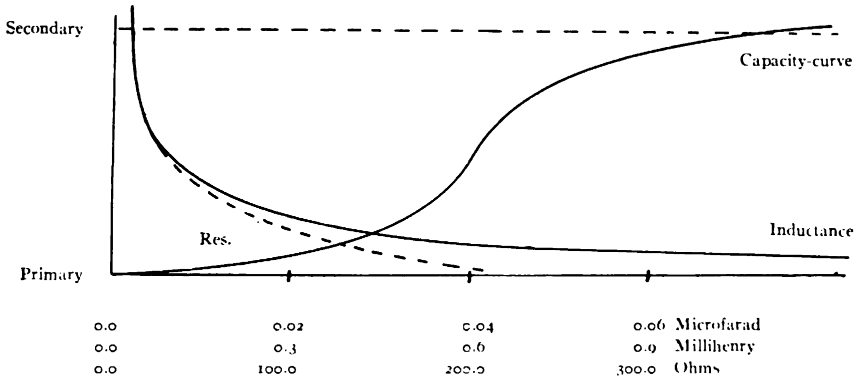


FIG. 1.

critical capacity independently of the presence of the other. In a previous paper[1] it was shown that the relative intensities of two *primary* spectra (of a mixture of gases) depend on the relative atomic weight of the component gases, while the relative intensities of the *secondary* spectra depend only on the relative numbers of the different kinds of atoms present. For example, it is easy to prepare a tube of mixed hydrogen and nitrogen that shows a ("four-line" secondary) hydrogen spectrum with capacity, but a nitrogen spectrum without. To detect spectroscopically, small amounts of a gas of low atomic weight mixed with a heavier, secondary spectra are most favorable while it is better to work with primary spectra when seeking evidence of a gas of large atomic weight. If an unknown gas exhibit two widely different spectra under different excitation, that is rather evidence that it consists of but a single gas than that it is a

[1] *Ibid.*, March 1904.

mixture of two different gases. When working with gases of large atomic weight, impurities of low atomic weight will be of little consequence, particularly if the primary spectrum is used.

To obtain a pure primary spectrum, then, it is necessary to avoid introducing capacity of any kind (particularly long lead wires) into the tube circuit. It is useless to attempt to neutralize the effect of even a small capacity by introducing inductance or resistance. To obtain a pure secondary spectrum, inductance and resistance are to be avoided, and at least 0.05 microfarads capacity must be added. With more than 10 ohms resistance or 0.01 millihenry inductance in the tube circuit, it is useless to attempt to get a pure secondary spectrum by simply adding an excess of capacity.

In a previous paper[1] the hypothesis was advanced that whole atoms radiate primary spectra, while atoms from which one or more electrons have been torn radiate secondary spectra. The atoms of the metallic elements appear to be so easily ionized that the feeblest current that will excite luminosity is more than sufficient to disrupt the atom, hence only electro-negative elements give both primary and secondary spectra. Large capacity would then produce always secondary spectra by increasing the intensity and suddenness of the current-wave through a gas, since a wave of large amplitude and steep wave-front would be vastly more effective in tearing apart an atom. Under these conditions we should expect inductance to tend to reduce secondary to primary spectra, since it reduces the slope of the current-wave; resistance would produce the same effect by lowering the wave-amplitude. Increasing the cross-section of the discharge would have the same effect as increasing the resistance. Critical capacity would be greater at the more refrangible end of the spectrum because modes of vibration of higher frequency would require a steeper current-wave to affect them; or perhaps it is equivalent to say that the smaller orbits are most stable. Critical capacity being a function of spectral wave-length, I consider as strong evidence that each spectral line is due to a different electron and *not* merely to one of many modes of vibration possessed by (say) a ring of electrons.

NATIONAL BUREAU OF STANDARDS,
Washington, D. C.,
June 1904.

[1] *Ibid.*, p. 243, May 1904.

FAINT STARS NEAR THE TRAPEZIUM IN THE *ORION* NEBULA.

By J. A. Parkhurst.

The lists of new variable stars in the Great Nebula of *Orion*, lately published by Wolf[1] and Pickering,[2] include none near the trapezium, as the faint stars are hidden by the brightness of the nebula on plates taken with short-focus instruments. Hence an examination has been made of the nine negatives taken by Mr. Ritchey with the Yerkes forty-inch refractor in 1900 and 1901. As these negatives were taken at the visual focus with a yellow screen and isochromatic plates, the photographic magnitudes will correspond quite closely with the visual values. The examination has been limited to the region within 2′ of arc of the trapezium star θ', and only those stars have been included in the list which appear on several of the plates, thus giving some basis for an opinion as to their variability. The exposure times of the plates range from one to four hours, but the stars are usually best shown with one-hour exposure.

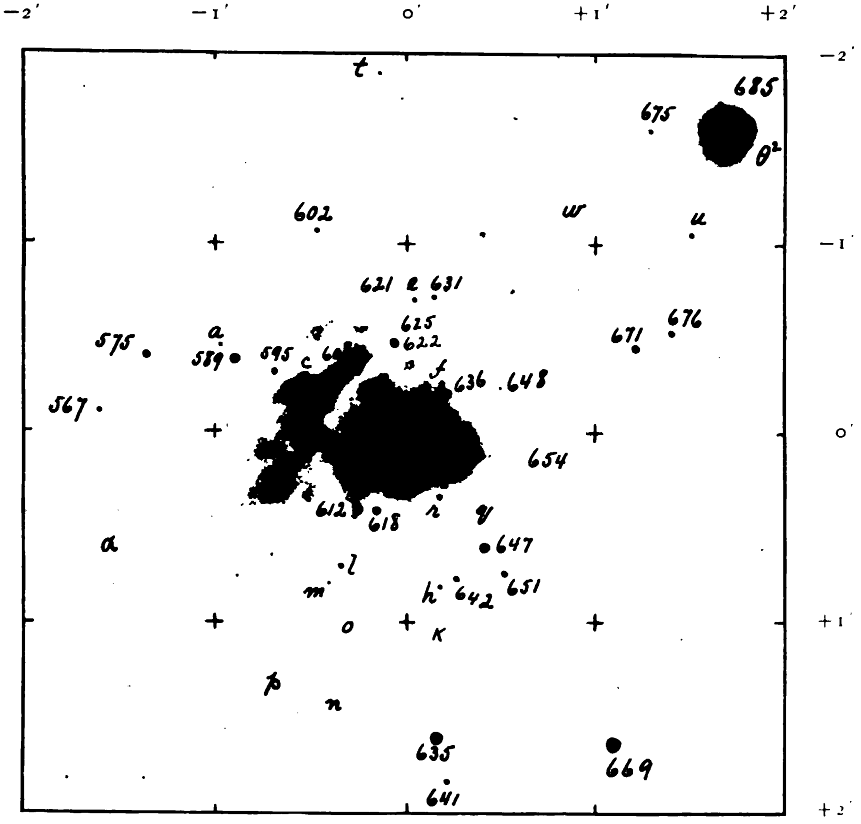
NOTATION AND POSITIONS.

Plate XII is an enlargement from the negative of 1900 October 17, $15^h\ 10^m$ to $16^h\ 10^m$, Central Standard Time. The orientation and scale were deduced from the positions of the stars Bond 558, 657, and 708, as measured by Scheiner[3] and reduced to the epoch of Bond's catalogue, 1857. Bond's numbers are used for all stars given by him, and letters are assigned by the writer to the faint stars not in Bond's list. As a check on the positions, a similar enlargement was made from the negative of 1901 Dec. 28, and the co-ordinates of the stars relative to θ^1 were measured with a millimeter scale on the two enlargements. The mean differences in the resulting positions were $0\overset{s}{.}7$ in R. A. and $0\overset{''}{.}5$ in Dec. (differences greater than $1''$ occurring only in case of faint and poorly defined stars), thus insuring correct identifications.

[1] *Astronomische Nachrichten*, **164**, 303, 1904.

[2] *Harvard Circulars*, **78**, **79**. [3] *Potsdam Publications*, **11**, 63, 64.

PLATE XII.



Faint Stars near Trapezium in Nebula of *ORION*.
(The numbers for the brighter stars are Bond's.)

Omitting the six trapezium stars and θ^2, a comparison of the number of stars shown or measured in this region by different observers follows:

TABLE I.

Bond....................	*Harvard Annals*, 5.........	visual 23
W. H. Pickering..........	*Harvard Annals*, 32........	photographical 10
E. C. Pickering...........	*Harvard Circulars*, 78, 79...	photographical 0
Scheiner..................	*Potsdam Publications*, 11...	photographical 0
Yerkes 40-inch...........	....	photographical 42

For convenience of comparison, the magnitude estimates are based on Bond's scale, though for the faint stars his numbers are probably too large, perhaps by 0.5 at fifteenth magnitude. These estimates are evidently subject to greater uncertainty than in case of stars in a region free from nebulosity, as the background varies much in density on plates of different exposure times. It should also be stated that Plate XII does not preserve the relative magnitudes of the outlying stars, as the reproduction was made to include as little nebulosity as possible.

Table II gives the results of the investigation, also for comparison Bond's co-ordinates and magnitudes, the headings of the columns being sufficient explanation of the contents.

VARIABLE STARS.

Bond or Struve has suspected of variability all the stars in this region visible to them except 625 and 635. The present investigation seems to prove the variation of 642 with a range of at least 2.5 magnitudes, and of 654 with a range of 4 magnitudes. Less certainly proved is the variability of 567, 589, 608, 625, 641, 675 and *s*, which with a few other stars require special mention.

Bond 601. Not seen except as a condensation in the nebula; only glimpsed by Bond on one night; perhaps is not a star.

Bond 608. In dense nebulosity; comparisons uncertain.

Bond 621 and 625. Only glimpsed by Bond on one night and positions estimated by him, not measured.

Bond 642. It is perhaps a question whether Bond saw this star or my *h*. At maximum the variable is brighter than *h*, but at minimum it is about 2 magnitudes fainter.

TABLE II.

Designation	Yerkes					Bond		
	Co-or. from θ^1		Magnitude			Co-or. from θ^1		Magnitude
	R. A.	Dec.	Mean	Brightest	Faintest	R. A.	Dec.	
567	−1′37″	−0′ 6″	13.4	13.2	13.6	−1′43″	−0′ 8″	13.9
a	−1 27	+0 36	15.8	....	....	......	......	....
575	−1 22	−0 23	11.8	....	....	−1 25	−0 22	11.9
u	−0 59	−0 27	15.0	....	....	......	......	....
589	−0 55	−0 23	var?	12.4	13.3	−0 57	−0 20	12.7
595	−0 42	−0 18	15.0	....	....	−0 47	−0 15	13.9
p	−0 37	+1 15	14.8	....	....	......	......	....
c	−0 32	−0 16	15.6	....	....	......	......	....
d	−0 27	−0 11	15.8	....	....	......	......	....
602	−0 28	−1 3	14.2	....	....	−0 33	−1 8	14.3
m	−0 24	+0 49	15.8	....	....	......	......	....
l	−0 20	+0 43	14.0	....	....	......	......	....
n	−0 20	+1 22	14.8	....	....	......	......	....
608	−0 19	−0 18	var?	14.4	15.2	−0 24	−0 18	14.3
o	−0 16	+0 58	15.8	....	....	......	......	....
612	−0 15	+0 25	13.3	....	....	−0 16	+0 25	13.5
618	−0 9	+0 26	13.3	....	....	−0 10	+0 25	13.1
621	−0 9	−0 41	15.8	....	....	−0 8	−0 36	15.6
t	−0 9	−1 53	14.2	....	....	......	......	....
622	−0 4	−0 27	12.7	12.0	12.9	−0 8	−0 28	12.7
625	−0 4	−0 31	var?	15.2	16.5	−0 4	−0 28	15.6
e	+0 2	−0 41	14.7	....	....	......	......	....
s	+0 7	0 0	15.6	....	....	......	......	....
631	+0 9	−0 42	14.1	....	....	+0 3	−0 42	14.3
k	+0 9	+1 0	15.6	....	....	......	......	....
635	+0 10	+1 37	9	....	....	+0 8	+1 38	10.5
h	+0 10	+0 50	14.7	....	....	......	......	....
r	+0 11	+0 21	16.0	....	....	......	......	....
j	+0 11	−0 14	16.1	....	....	......	......	....
641	+0 13	+1 51	13.1	12.6	13.3	+0 12	+1 51	14.8
636	+0 13	−0 12	13.8	....	....	+0 8	−0 9	13.3
642	+0 16	+0 47	var.	14.0	16.6	+0 13	+0 48	15.6
q	+0 19	+0 30	16.8	....	....	......	......	....
647	+0 25	+0 37	11.2	....	....	+0 23	+0 38	12.1
648	+0 30	−0 13	14.4	13.6	14.7	+0 24	−0 9	14.3
651	+0 31	+0 45	12.6	....	....	+0 29	+0 48	13.1
654	+0 36	+0 8	var.	11.9	16.1	+0 33	+0 10	12.3
w	+0 52	−1 3	15.9	....	....	......	......	....
671	+1 12	−0 26	11.7	....	....	+1 10	−0 24	11.5
675	+1 17	−1 35	14.6	14.4	14.8	+1 15	−1 33	15.2
676	+1 24	−0 31	12.7	....	....	+1 19	−0 28	13.1
u	+1 30	−1 2	14.5	....	....	......	......	....

s. This is a double star discovered by Barnard with the Lick 36-inch in 1888. It is called "excessively faint" by Burnham,[1] the components being rated as 16.0 and 16.5 magnitude. It is well shown on the negatives of one-hour exposure, and, if of ordinary color, seems to be at least a magnitude brighter than in 1888. It is too near the trapezium to be shown on the print.

Bond 567. Rated by Holden as 16.3 magnitude, 1878 Jan. 5, with Washington 26-inch.[2]

Bond 641. Rated by Holden as 16 magnitude, 1874 Jan. 6.[3]

YERKES OBSERVATORY,
July 28, 1904.

[1] *Publications of the Lick Observatory*, 2, 48.

[2] *Washington Observations*, 1878, *Appendix I*, 182.

[3] *Ibid.*, page 181.

ON SOME RESULTS OBTAINED BY THE D. O. MILLS EXPEDITION TO THE SOUTHERN HEMISPHERE.[1]

By W. H. WRIGHT.

THE Observatory of the D. O. Mills Expedition to the Southern Hemisphere was installed on the summit of Cerro San Cristobal in the city of Santiago de Chile during the southern winter of 1903, and observing was commenced on September 11. In advance of a detailed description of the observatory, instruments, and methods of work, which will be forthcoming, it is desirable to publish certain results secured during the progress of the work. It will be sufficient for present purposes to state that the equipment of the expedition includes a reflecting telescope of 94 cm clear aperture, Cassegrainian mounting, and a powerful three-prism spectrograph. With this combination, results are being secured comparable in point of accuracy with those obtained with the Mills spectrograph at Mount Hamilton. Three hundred and eight successful spectrograms were obtained up to June 1, 1904.

The following stars have been found to have variable radial velocities:

β Doradus ($\alpha = 5^h\ 32^m.7$; $\delta = -62°\ 33'$).

Date	Velocity	Measured by
1903, Sept. 20	+ 1.4 km	Palmer
Dec. 21	+16.1	"
1904, Jan. 12	+28.0[2]	"
Jan. 22	+28.5	"

w Velorum ($\alpha = 8^h\ 56^m.3$; $\delta = -40°\ 52'$).

1904, Jan. 21	+ 3.0[2] km	Palmer
Mar. 8	+13.7	"
Mar. 28	+ 7.5	"

[1] Also to appear as a *Bulletin* of the Lick Observatory.

[2] Rough measurements of very poor plates.

l Carinae ($\alpha = 9^h\ 42^m.5$; $\delta = -63°\ 3'$).

This is a variable star having a period, according to A. W. Roberts,[1] of 35.523 days. The light-variation, according to the same authority, is irregular. Our observations are as follows:

Date	Velocity	Measured by
1904, Apr. 18	+10 km	Wright
Apr. 30	+22	"
May 8	−15	"

κ Pavonis ($\alpha = 18^h\ 46^m.6$; $\delta = -67°\ 21'$).

This is also a variable star, having, according to the same authority, a regular light-curve, and a period of 9.091 days.

Date	Velocity	Measured by
1904, May 12	+40.1 km	Palmer
June 6	+28.9	"
June 22	+26.5	"

τ Sagittarii ($\alpha = 19^h\ 0^m.7$; $\delta = -27°\ 49'$).

1902, Aug. 17	+34.0 km	*
1904, May 12	+51.0	Palmer
June 7	+59.7	"

The variations in the velocities of *β Doradus* and *w Velorum* were detected by Dr. Palmer.

Observations of the radial velocities of the components of *α Centauri* have been secured, as follows:

α_1 *Centauri* (fainter component).

Date	Velocity	Measured by
1904, Feb. 25	−18.90 km	Wright
Mar. 4	−18.00	"
June 23	−19.70	"

[1] *Astronomical Journal*, **21**, 81, 1901.

* Observation made at Mount Hamilton.

α_2 *Centauri.*

1904, Feb. 21.................	−24.02 km	Wright
Mar. 4.................	−24.20	"
June 23.................	−24.58	"

In addition to these observations, the spectra were photographed on May 29, using a short camera designed for work on fainter stars. The negatives are rather poor, the spectra being over-exposed in the region of good definition. However, they were measured soon after being taken. The results are:

α_1 *Centauri*		α_2 *Centauri*	
1904, May 29	−19.70 km	May 29	−24.30 km

For reasons stated, it was considered that these observations would not add to the value of the final determination. They have accordingly not been used.

A difference in the radial velocities of the components is shown to exist.

In order to make the observations as nearly as possible differential, exposures after that of February 25 were made on the two components in quick succession, the adjustments and the settings of the instrument remaining unchanged. The differences taken by pairs are:

Date	V_1-V_2
1904, February 21–25 - - - - -	5.12 km
March 4 - - - - -	5.51
June 23 - - - - - -	4.88
Mean - - - - - -	5.17 km

As to the cause of the difference, two assumptions may in general be made in a case of this sort.

1. The difference may be due to the relative orbital motion of the two components.

2. It may be due, in part at least, to one of the components being a spectroscopic binary.

Under the first of these hypotheses the parallax of α *Centauri* may be computed, as the visual orbit of the pair is accurately known. Assuming the elements determined by Roberts,[1]

[1] *A. N.*, **133**, 105, 1893; also **139**, 7, 1895.

$$
\begin{aligned}
T &= 1875.715 \\
P &= 81.185 \text{ years} \\
e &= 0.52865 \\
\lambda &= 52^\circ\ 0'\ 58'' \\
i &= 79^\circ\ 21'\ 36'' \\
\Omega &= 25^\circ\ 5'\ 50'' \\
a &= 17''.71
\end{aligned}
$$

the computation is readily effected by means of the following formulæ adapted from the work of Lehmann-Filhés:

$$n = \frac{2\pi}{86400 \times 365.26 \times P},$$

$$a = \frac{\Delta V \sqrt{1-e^2}}{n \sin i \,[e \cos \lambda + e \cos (V+\lambda)]},$$

$$\pi'' = \frac{a''}{a} R,$$

$$m_1 + m_2 = \frac{a^3}{R^3 P^2};$$

where

R = mean heliocentric distance of the Earth in kilometers,

a = mean distance of the components of α *Centauri* in kilometers.

n = mean angular motion of α *Centauri* in circular measure, per second of time.

ΔV = observed difference in radial velocity of the two components.

m_1 and m_2 = masses of α_1 and α_2 *Centauri* in terms of the mass of the Sun.

Dr. Palmer has at my request performed the computations. His results are

$$
\begin{aligned}
\pi &= 0''.76\ ,[1] \\
a &= 3.46 \times 10^9\ , \\
m_1 + m_2 &= 1.9\ .
\end{aligned}
$$

The computed probable errors for these quantities, depending on the residuals from the mean of the three determinations of differential velocity are, for π, $\pm 0''.03$, and for $m_1 + m_2$, ± 0.2. Being based

[1] Gill and Elkin's value of the parallax of α *Centauri*, from heliometer observations is

$$\pi = 0''.75 \pm 0''.01,$$

relative to the comparison stars used, of average magnitude 7.6.

on such a small number of observations, the weights are quite uncertain. Furthermore, they do not take into consideration the uncertainties in the elements of the orbit, nor other sources of error to be discussed later. In this connection it should be remembered that the orbit of *α Centauri* is among the most accurately known of double star orbits.

The second hypothesis, while not a probable one, cannot in general be neglected in a discussion of this nature. Radial velocity determinations on an extensive scale have developed the fact that at least one star in every seven has a variation in velocity great enough to be detected by the powerful spectrographs now in use. If among m stars there are n spectroscopic binaries, then the chance that of any two taken at random at least one should belong to this class is

$$\frac{n(2m-n-1)}{m(m-1)}$$

which, if m be large, reduces approximately to

$$\frac{n}{m}\left(2-\frac{n}{m}\right),$$

Assuming $\frac{n}{m}=\frac{1}{7}$, it appears that the probability of at least one of a pair of stars being a spectroscopic binary is a little greater than one-fourth. The probability in the case under discussion is somewhat lessened by the fact that the general ratio of one to seven is influenced to a great extent by short-period variation, which is not shown to exist by these observations extending over four months. At the same time, it must be confessed that we are working very much in the dark, as the ratio may be different in the cases of telescopic double stars from what it is for stars apparently single. But be the probability great or small, the actual existence of variable velocities in the components of *ζ Ursae Majoris*, *α Geminorum*, *κ Pegasi*, and other well-known double stars warns us to accept with some reservation parallax determinations based on observations of radial velocity extending over only a short period of time.

It seems advisable to publish these observations now, as it is likely to require a great many years to determine whether the observed differences in radial velocity vary according to the stars' orbital motion.

It is to be noted that considerations similar to the foregoing apply with equal force to the older and more direct method of determining parallax, as the dimensions of the orbits of many spectroscopic binaries are of the order of magnitude of those of the Earth's orbit. In determining by either method it is therefore desirable that the observations should extend over a considerable period of time.

In conclusion, I may be permitted to recapitulate the advantages of the spectroscopic method (where applicable) over the direct method.

1. No assumption is made as to the great distance of certain comparison stars.

2. The accuracy of the determination of the star's distance is to a certain degree independent of this distance.

3. The quantities upon which the parallax depends will usually be of an order lower than that of the probable error of measurement.

The brighter component of *α Centauri* has a solar-type spectrum. In the spectrum of the fainter component the heavy iron lines are much more pronounced, and the calcium absorption is exceedingly heavy. According to Roberts,[1] the masses of the stars are nearly equal, the exact ratio being $\frac{51}{49}$ in favor of the brighter component. When the relative velocity in the line of sight shall have changed, this ratio can be determined from velocity observations, though probably not with such accuracy as from heliometer measurements from neighboring stars.

OBSERVATORY OF THE D. O. MILLS EXPEDITION
TO THE SOUTHERN HEMISPHERE,
Santiago de Chile, June 28, 1904.

[1] *A. N.*, **139**, 10, 1895.

Minor Contributions and Notes.

A LIST OF FIVE STARS HAVING VARIABLE RADIAL VELOCITIES.[1]

In the course of the line-of-sight work with the Lowell spectrograph, the following five stars have been discovered to be spectroscopic binaries. These are additional to those previously announced. Since some of the plates employed in these determinations have been incompletely measured and reduced, the time is given only to the day. The letters S and L before the plate numbers refer respectively to the short and to the long camera.

α Andromedæ ($\alpha = 0^h\ 3^m.2$; $\delta = +28°\ 33'$; Mag. = 2.1).

The following observations of the radial velocity of this bright star show it to be a spectroscopic binary.

Plate	Date	Velocity
S 470	1902 Oct. 30	−40 km
L 1248	1903 Nov. 25	−42
L 1253	Nov. 26	−40
L 1290	Dec. 1	−34
S 1306	Dec. 14	−27
S 1318	Dec. 19	−24
S 1328	1904 Feb. 10	+16
S 1331	Feb. 11	+20
S 1338	Feb. 17	− 5
S 1341	Feb. 29	−37
S 1347	Mar. 4	−45
S 1349	Mar. 6	−44
S 1402	May 22	+10

These velocities depend principally upon displacements of the hydrogen line $H\gamma$ and the magnesium line λ 4481. The helium line at λ 4472 is measurable on a few of the plates. Owing to the character of the spectrum and the poor quality of some of the plates, these values for the velocity may be in error a few kilometers.

This star was observed by Vogel and Scheiner in 1889.93 to have a velocity of +4.5 km, and when plate L 1248 was found to give a velocity differing by 45 km, the binary character of the star seemed certain. (Plate S 470 had not been previously measured owing to a badly overexposed

[1] *Lowell Observatory Bulletin* No. 11.

comparison spectrum.) These observations seem to indicate a period of about one hundred days, and a highly eccentric orbit.

α Librae ($\alpha = 14^h\ 45^m.4$; $\delta = -15°\ 37'$; Mag. = 2.3).

The observations of this bright star showing the variation in its radial velocity are the following:

Plate	Date	Velocity
S 1406	1904 May 24	−60 km
S 1460	June 21	−20
S 1482	June 27	+ 4
S 1500	July 6	+20

The spectrum of this star is somewhat more advanced than that of *Sirius*, and is quite similar to that of *α Piscis Austrini*. There are numerous metallic lines, but they are poorly defined and not suitable for accurate measurement. The appearance and behavior of the hydrogen line $H\gamma$ suggest that both components are bright.

σ Scorpii ($\alpha = 16^h\ 15^m.1$; $\delta = -25°\ 21'$; Mag. = 3.0).

The variation in the radial velocity of this star was discovered from the second plate. The observations are as follows:

Plate	Date	Velocity
S 1451	1904 June 18	−25 km
S 1475	June 25	+25
S 1481	June 26	+17
S 1506	July 7	− 5

The spectrum is of the *Orion* type, and the lines are quite well defined.

X Sagittarii ($\alpha = 17^h\ 41^m.3$; $\delta = -27°\ 48'$; Mag. = 4.9).

This is the visual variable *X Sagittarii*, having a period of seven days. Only two spectrograms of the star have thus far been secured. They give the following values for the radial velocity.:

Plate	Date	Velocity
S 1455	1904 June 19	+ 1 km
S 1464	June 22	−22

This range is not great, but the spectrum contains many well defined lines, and there is no reason for doubting the reality of the variation in the

velocity. The spectrum is intermediate between that of α *Canis Minoris* and that of the Sun.

ϵ *Capricorni* ($\alpha = 21^h\ 31^m.5$; $\delta = -19^\circ\ 54'$; Mag. $= 4.5$).

The variable velocity of this star was discovered in October 1903 from the fourth plate. The observations thus far obtained are the following:

Plate	Date		Velocity
S 1004	1903	Aug. 21	−40 km
S 1015		Aug. 24	−42
S 1046		Sept. 7	−45
S 1166		Oct. 28	−16
S 1170		Nov. 2	−15
L 1233		Nov. 22	−27
S 1469	1904	June 23	−23
S 1499		July 5	+ 1
S 1504		July 6	+ 6

The spectrum of this star is of the *Orion* type and is peculiar. The hydrogen line $H\gamma$ is, in general, very sharply defined, and the determinations depend principally upon the measures of this line alone. On some plates other ill-defined lines appear, and it may be that both components are bright.

V. M. SLIPHER.

JULY 8, 1904.

THE

ASTROPHYSICAL JOURNAL

AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME XX OCTOBER 1904 NUMBER 3

I. A METHOD OF MEASUREMENT AND REDUCTION OF SPECTROGRAMS FOR THE DETERMINATION OF RADIAL VELOCITIES.

II. APPLICATION TO A STUDY OF THE VARIABLE STAR *W SAGITTARII.*[1]

By RALPH H. CURTISS.

INTRODUCTION.

BEGINNING thirty-five years ago, when Huggins and Vogel first made application of Doppler's principle to the determination of radial velocities of stars, the methods employed in this work have been developed, until at present an efficient and practically uniform system has been adopted in the various astrophysical observatories. In 1888, when the attempt was made at Potsdam to record photographically the displacement of lines in the stellar spectra, the problem was placed on a firm basis. Subsequently, through improvement in the construction of spectrographs, a more accurate knowledge of the wave-lengths of spectral lines, and signal advances in the methods of measurement and reduction of the spectrograms, the results have attained their present degree of accuracy. The probable error of 22 km per

[1] Dissertation in partial fulfilment of the requirements for the degree of doctor of philosophy in the University of California. Also to appear as a *Bulletin* of the Lick Observatory.

second for an average case of velocity determination with the older methods has been reduced to a few tenths of a kilometer at the present time.

Flexure and temperature-change during exposure may be said to play no part in the performance of the modern properly equipped spectrograph, and other observational sources of error are well under control. Undoubtedly, the spectrograms of today contain information regarding the velocities of celestial light-sources more precise than our present methods of measurement and reduction are capable of bringing out. In these methods there exist certain recognized sources of accidental and systematic error which must be eliminated if the coveted hundreth of a kilometer can ever have any significance.

Evidences of the extent of uncertainty which characterizes the spectrographic results of the last eight years can be obtained by comparison of determinations of velocity from the same star with different instruments. In earlier work strong systematic differences were encountered. In measures of the star *ζ Herculis*[1] the difference Campbell (5 plates)-Bélopolsky (5 plates) = -3.5 km. Two more plates by Bélopolsky greatly diminish this difference, but increase the probable error of his mean so signally that their inclusion is hardly consistent. For *ζ Geminorum*[2] the residual Campbell (44 plates) − Bélopolsky (15 plates) = -6 km. For *α Persei*[3] the residual Campbell (4 plates) − Vogel (13 plates) = -0.8 km. In the case of *Polaris*,[4] Hartmann finds: Campbell − Bélopolsky = -3 km, and for Campbell-Frost = $+2.2$ km. For *Capella*[5] the difference Campbell (31 plates) − Newall (23 plates) = $+3$ km. For purposes of this comparison the investigations of standard velocity stars have not progressed far enough as yet. But from observations made at Yerkes Observatory Frost and Adams conclude that "there would appear to be a slight tendency toward a systematic difference between our results and those of other observers in the direction of a larger positive, or a smaller negative, value for our velocities."[6] That any large systematic discrepancies will be brought out by these

[1] ASTROPHYSICAL JOURNAL, **8**, 157, 1898.

[2] *Ibid.*, **13**, 90, 1901.

[3] *Ibid.*, **13**, 320, 1901.

[4] *Ibid.*, **14**, 52, 1901.

[5] *Ibid.*, **12**, 251, 1900.

[6] *Ibid.*, **18**, 276, 1903.

results from standard stars is highly improbable. The instruments involved have been in use long enough to be thoroughly under control, and the choice of lines from the star and check-plates would naturally be governed by their performance in previous comparisons with the theoretical velocities of the Moon and planets. An uncertainty of at least several tenths of a kilometer, which, I think, is largely attributable to sources of error in the treatment of the plate, undoubtedly remains in spectrographic velocity determinations.

I. METHODS OF MEASUREMENT AND REDUCTION OF SPECTROGRAMS.

The plan of procedure that has been widely adopted, subject to slight variation in the case of different observers, may be briefly described. The spectrogram is measured twice, direct and reversed, on an engine supplied with a microscope of adjustable magnifying power, rigidly mounted over a plate-carriage which is moved by a delicate screw and micrometer system in one co-ordinate only, in the direction of extension of the spectrum. A set of measures includes the list of micrometer readings which correspond to the positions of the plate-carriage when the various spectral lines, both bright and dark, occupy in turn a given position with reference to some fixed point in the focal plane of the microscope. These readings, together with the laboratory determinations of the wave-lengths of the spectral lines employed for the measurements, constitute the data necessary for the determination of the radial velocity of the source emitting the light in terms of the accepted standards of velocity. From the micrometer readings and wave-lengths corresponding to not less than three known lines in the comparison or solar spectrum, the constants of the Hartmann–Cornu interpolation formula[1] are easily computed. By this formula micrometer readings are readily transformed into approximate wave-lengths, and *vice versa*. If, now, each plate is to be reduced independently, as at Yerkes Observatory,[2] the constants of the empirical interpolation curve are computed as above from three or more comparison lines. Corrections to this

[1] ASTROPHYSICAL JOURNAL, 8, 218, 1898; *Astronomische Nachrichten*, 155, 81–118, 1901.

[2] *Publications of the Yerkes Observatory*, 2, 150, 1903; ASTROPHYSICAL JOURNAL, 15, 22, 1902.

curve are determined by comparison of the adopted and interpolated wave-lengths, and are plotted as functions of the micrometer readings or the wave-lengths themselves. It then remains to compute by means of this corrected interpolation formula the wave-lengths corresponding to the micrometer settings on the lines of the spectrum to be measured. We thus obtain the actual wave-lengths of these lines as affected by the motion of their source relatively to the observer. From the known wave-lengths of these lines, occurring in the spectrum of a light-source at rest with reference to the observer, a simple application of Doppler's principle will yield the desired radial velocity of the star. If, on the other hand, the dispersions of one or more fundamental solar plates[1] be taken as standards, by measures on the comparison lines of a stellar spectrogram, the micrometer readings on the star lines themselves are reduced to the selected fundamental dispersion and may be compared directly with the computed readings of the same lines in the fundamental table. In point of accuracy, there seems to be no choice between these two methods, but the latter one is undoubtedly shorter. The sources of error, which are shared by both, I shall briefly review.

SOURCES OF ERROR IN THE REDUCTION AND MEASUREMENT OF SPECTROGRAMS.

a) *Change in film.*—The actual shape of the photographic film while the plate is being exposed is never reproduced, owing to distortions ascribable to temperature change and the processes of development and drying; but the effect of these distortions on velocity determinations is minute and intangible, especially when many well distributed comparison lines are employed.

b) *The measuring engine.*—(1) Errors in screw. Errors due to inequalities in the pitch of the micrometer screw, whether periodic or otherwise, are usually negligible in modern engines. They may, of course, be eliminated by application of small corrections to the measures, but are essentially inoperative in most velocity work.

(2) Temperature change during measures. The effect of temperature change on the measuring engine during measurements may play an important part unless considerable care is exercised. Meas-

[1] *Astronomische Nachrichten*, **155**, 101, 1901; ASTROPHYSICAL JOURNAL, **8**, 124, 1898.

ures should not be begun until the micrometer-head has been in contact with the hand for a short time, and during the measures the room temperature should be kept nearly constant. In my own work, after completing a plate I have tested the temperature shift by setting back on the lines first measured, and have invariably found some discrepancy. In one solar plate in particular, which had required considerable time for measurement, this shift amounted to about 0.003 mm, or the equivalent of seven kilometers' velocity. This led me to test the character of the shift by remeasurement of some of the remaining comparison lines on the plate. The plot of differences thus obtained approximated a straight line which gradually approached the original curve. Evidently the temperature-change was progressive and was entirely taken up in the first curve. Here, again, the advantage of an even distribution of star and comparison lines is apparent. Evidently, also, the "smoothing out" of curves of residuals from interpolation formulæ should not be practiced without due caution, for temperature-change during the measures may produce very appreciable distortions in such a plot.

c) *Subjective errors.*—Discrepancies arising from accidental errors of setting and from personal equation are among the most important to be considered. The former can be reduced only by long practice and the employment of a greater number of lines; the latter have been largely eliminated by the reversal of the spectrogram on the engine,[1] but it should be noted that in the reversal of a plate the spectrum is simultaneously inverted. Thus, in general, it would hardly be said that the effect of personal equation is entirely compensating in the two measures, for the appearance of a line after inversion might be so changed as seriously to interfere with the duplication of the conditions which existed before reversal. Further, in case of a straight-slit spectrograph the curvature[2] of the spectral lines would seriously interfere with the elimination of personal equation in this way.

d) *Errors due to the assumption of wave-lengths.*—(1) An instrumental defect. A source of error by no means inappreciable is found

[1] *Lick Observatory Bulletin* No. 15, and ASTROPHYSICAL JOURNAL, **15**, 208, 1902.

[2] Curved slits are used on the Mills spectrographs. They give straight spectral lines, which is a decided advantage.

in the relative displacement of comparison and absorption lines due to imperfect focal conditions which obtain outside of given narrow regions of the spectrum corresponding to the rays which pass through the prism-train at minimum deviation, or corresponding to the intersections of the focal curve with the plane surface of the photographic plate. The so-called wings, which seriously impair the definition of the outlying lines of the comparison spectrum, undoubtedly persist far into the region of apparently sharp focus, giving rise to errors in the measured positions of the bright lines. The corresponding displacements of the absorption lines of the stellar spectra, due to the same cause, are certainly much smaller, if at all appreciable. This difference is due to the fact that much light is concentrated in the bright lines, many of which are strongly exposed in order to bring out the fainter features of the comparison. Thus the wings, though they share a relatively small part of the light, may become quite as dense as the sharp part of the lines, for the density of the sharp part of the lines is but slightly affected by light after a certain time, while the wings continue to spread.

The nature of the effect of this source of error is difficult to predict, for it depends entirely upon the optical parts of the spectrograph and the character of the adjustments. If the focal curve of the camera lens is tangent to the plane of the surface of the photographic plate in the region of the spectrum corresponding to minimum deviation, the error arising would certainly differ from that which exists when the focal curve intersects the plate in two lines, though the character of the difference would be very uncertain. The results of a few investigations of actual measures with various instruments appear in the accompanying table. For each set of measures the means were formed of the velocity in kilometers as obtained from a given number of lines to the violet, to the red, and at minimum deviation. These means are indicated by V, R, and M, respectively, and the means of the differences between these quantities for an entire set of plates are given under the heads V–R, V–M, and R–M. The number of lines used on each plate in the formation of the means V, R, and M is given in another column, and the approximate range in wave-length between the means V and R appears under the heading "Range." The probable errors of the quantities determined are also added.

TABLE I.

Spectrographs	V–M	R–M	V–R	No. Lines on Each Plate	No. of Measures of Plates	Range
Bruce	....	....	+0.16km ± 0.08	6	84	98μ
Pulkowa	....	....	−0.85 ± 0.20	6	35	200μ
Mills	−1.76km ± 0.16	+1.10km ± 0.20	−0.66 ± 0.14	30	20	180μ

The results for the Bruce and Pulkowa spectrographs were obtained from data recently published in connection with the velocity determinations of standard stars. The measures of Frost and Adams for each plate were reduced separately. The results for the Mills spectrograph were obtained from a set of measures of negatives of *Polaris*, which afforded better material than the data used for the other instruments. The range covered by the Yerkes measures is so small that no marked effect could be expected, but for the other instruments there seems to be a fairly consistent difference between measures of lines in different parts of the spectrum. These results may be masked by errors in wave-lengths employed, but the discrepancies are strong enough to call for further investigation.

(2) Physical differences. Aside from subjective errors of measurement, there is no greater source of uncertainty in the reduction of spectrograms than that which arises from the assumption of wave-lengths for the star and comparison lines from Rowland's solar tables. It is doubtful if the conditions of density and internal motion in the Sun and spark are sufficiently alike to warrant this procedure. But until better determinations of spark wave-lengths are available, there seems to be no alternative. A careful discussion of the matter by Frost and Adams[1] has led to the conclusion that

In the present state of our knowledge we . . . . cannot say with any certainty how much our results are affected by the use of solar wave-lengths for our *Ti* lines; but presumably by an amount corresponding to less than 0.02 tenth-meters, or about 1.4 kilometers, and perhaps very much less.

Wave-lengths in star and Sun may differ by small amounts, due to

[1] *Publications of the Yerkes Observatory*, **2**, 155.

pressure, etc., but it seems practicable to assume agreement in the case of solar-type stars. In other cases the chances are favorable.

(3) Errors in Rowland's table. Rowland's determinations of solar wave-lengths are probably relatively accurate to the nearest hundredth of an Ångström unit. Their absolute value need be known only approximately for velocity work.

(4) Errors in practice. In actual performance some apparently good lines are found to yield consistently poor results, which usually leads to their final rejection. Errors in wave-lengths or intensities in Rowland's table, or close lines in the star, may be responsible for the trouble. However that may be, there is undoubtedly among the lines retained a large percentage which are in error similarly, but in a smaller degree. It simply remains to eliminate such lines as soon as they can be detected, though the uncertainty of proceeding in the dark in this regard is far from satisfactory.

There is a possibility of actual introduction of this form of error in the use of blends formed by weighting lines according to their intensities in Rowland's tables. The occurrence of enhanced lines is one great obstacle. But, further, in determining wave-lengths by blending, it must be remembered that the intensities of Rowland's lines were intended only for purposes of identification, and were not prepared with sufficient care to warrant more exacting applications. The characteristics as regards sharpness of lines comprising a blend would undoubtedly affect the apparent blended wave-length, though such characteristics could not be included in intensity estimates without considerable difficulty. Again, an intensity 0 applies to all lines between $a+0.5$ and $a-0.5$ in Rowland's scale; or, in other words, the uncertainty of any intensity in Rowland's table amounts to ± 0.5 of a unit. The possible error in a blend of this kind is well illustrated by the well-known combination of *Ti* (2), 4427.266 and *Fe* (5), 4427.482 into a line of wave-length 4427.420. If, however, the two intensities are 1.5 and 5.5, or again 2.5 and 4.5, the resulting wave-lengths are 4427.436 and 4427.405 respectively. Thus, under the assumption that the intensities in Rowland's table are as accurate as possible, an uncertainty of 0.015 tenth-meter (the equivalent of 1 km in velocity) must be recognized in the blend. It would therefore seem advisable with present methods to confine one's choice to the

seventy-five or eighty good single lines that are available in most high-dispersion instruments, rather than to include tempting groups whose wave-lengths are subject to uncertainties in addition to those incident to the laboratory determinations.

e) *Elimination of these errors.*—For errors due to change of film and inaccuracies in the measuring engine, including temperature-change during measures, adequate remedy has been proposed, but no elimination of the remaining uncertainties incident to present methods seems possible without a radical change in the system employed. As long as the wave-lengths determined from measures with the grating or interferometer by an observer of one personal equation, are employed by another observer of a different personal equation to represent his measures of corresponding lines as produced by a different spectroscope, it can hardly be expected that the results of various observers will be entirely consistent. The problem demands that the conditions which obtain in the production, measurement, and reduction of a spectrographic plate of a star should be exactly duplicated in the production, measurement, and in general the reduction, of the plate from which the fundamental data for velocity determinations are secured. The same spectrograph should produce both plates. They should be measured by the same observer with the same measuring engine. And, finally, they should be reduced in parallel. These requirements are clearly most exacting, but as a suggestion of a means toward this end I propose the following method, which, as far as I know, has never been applied in this way.

A PROPOSED METHOD FOR THE MEASUREMENT AND REDUCTION OF SPECTROGRAMS.

Proceeding as in case of a stellar exposure with the spectrograph in final adjustment from which it must not be disturbed, a source of accurately known velocity, such as the sky or Sun, is photographed with the comparison in the usual manner. Then the measures upon the comparison and continuous spectra of this plate with some engine by any observer will constitute his fundamental standard table for the engine and spectrograph used. These measures of the bright and dark lines of a plate of the sky or Sun and spark will fix the relative positions of the Fraunhofer and comparison lines for a known velocity, and will constitute a velocity-standard table. For the

determination of the velocity of any celestial object of the same characteristics as the standard source it is necessary only to duplicate the exposure and measures of the standard plate and compare directly the relative positions of corresponding lines of the two sources referred to the comparison spectrum of each plate. In the reduction of a stellar plate for velocity determination, we first reduce the star measures to the dispersion of the standard plate by forming a simple plot with micrometer readings as abscissæ and differences in settings on comparison lines as ordinates. For the star lines the reductions to fundamental dispersion are read directly from this plot. If these reductions are applied to the readings on the stellar spectrum, the measured positions of any line on the Sun and star plates will differ by an amount proportional to the relative velocity toward the Earth of the star and Sun.

The assumptions in this method are fundamental in every case of velocity determination. Further assumptions treated above as incident to other methods are here eliminated by the comparison of artificial lines with artificial lines, and dark lines with dark lines. The advantages of this simple system need only be mentioned. Errors in the screw of the measuring engine are eliminated for small displacements, such as are found in stellar spectra, if the standard and star plates occupy identical positions on the plate-carriage during measures. Personal equation is well controlled without the reversal of the spectrogram, and may be closely followed by frequent measures of standard plates. It may even seem preferable not to reverse the plate, for, as I have suggested, the curvature of lines may introduce a variation of this unaccountable source of error. The magnitude of accidental errors of setting is not affected by the use of this method of relative measures, but with the greater number of lines available in solar-type stars particularly, the reliability of the means from any plate is clearly increased. Uncertainties due to imperfect focal conditions are not only largely eliminated if the exposures on comparison and star are consistent in star and standard plates, but the region of spectrum available for measurement can be extended without fear of encountering difficulty. The number of lines available is further augmented for solar-type stars by the possibility of including blends, which are clearly reliable under this method. Until tests

are made with high-dispersion instruments it is difficult to predict the extent of the advantage of this increase of the measurable lines for this case; but for low-dispersion instruments, in my own experience with a magnifying power of 25, the number of available lines increased from 20 to 170; and with a power of 10, though practically no lines could be used with methods depending on wave-lengths, 65 lines gave good results for velocity determinations. But for the present the chief advantage of the method lies in its independence of the relative or absolute values of the wave-lengths or intensities of the spectral lines of the spark or continuous spectrum. For the determination of the factors necessary to convert micrometer displacements into kilometers, rough relative values of the wave-lengths of three favorable spark lines are needed; but, aside from this, there is no necessity for data of this kind. It is possible to make final definitive measures at once.

In the application of this method any degree of refinement can be introduced at the will of the observer. The number of plates employed to form a standard table can be increased until the full possibilities of the spectrograph and measuring engine are realized. By comparison of standard plates any errors due to temperature-change in the instrument can be detected, and tables can be prepared for different temperatures, if desired. Standard plates can be prepared occasionally and measured, along with the regular observing list, as checks on the constancy of the results. For absolute checks of the performance of the method, lunar and planetary spectrograms are of course available, but the determination of the sky or solar velocities furnish equally reliable checks, and at the same time afford data for strengthening the original standard tables. In general, three sky or Sun plates should furnish sufficient data for a fundamental table, though the conditions obtaining in any particular case might call for a greater or smaller number.

As a source of known velocity for the standard plates, the Sun is perhaps superior to the sky, but if exposures on the sky are more convenient, there should be little, if any, hesitation in their use. The objection to the use of sky-light lies in the fact that the lines in its spectrum are subject to a slight broadening due to the Sun's rotation. This would hardly exceed 0.020 tenth-meters, which would

occasion no impairment of the measures of lines which in most favorable cases are many times this amount in width. As star-light itself possesses the integrated characteristics of sky-light, it may seem preferable to use the sky in the preparation of standard plates.

This velocity-standard method is best applicable to solar-type stars, but it may, if desired, be modified to suit the requirements of hydrogen or other stars without the introduction of errors greater than those incident to present methods. Thus a standard table for any class of stars may be prepared by introducing in the spark, in addition to the regular comparison, those elements which produce good lines in the stellar spectra. The assumption is made that the relative positions of the spark and dark lines of the elements used are not affected differently either by the dissimilar physical conditions in the spark and star or by the peculiarities of the spectrograph; but this is an assumption commonly made in all spectrographic work.

II. AN APPLICATION OF THE VELOCITY-STANDARD METHOD.

(*a*) *Apparatus employed.*—Necessity for some departure from the present methods of measurement and reduction of spectrograms first arose in the attempt to improve the accuracy of relative velocity determinations from spectrum plates made with Spectrograph I of the Lick Observatory. The performance of this instrument is well known in connection with the spectrographic studies by Campbell, Wright, H. D. Curtis, and Stebbins, of the spectra of *Nova Persei*, *Nova Geminorum*, and *o Ceti*, and with velocity determinations by Campbell of *1830 Groombridge*. As the result of this work, Professor Campbell was led to remark that

> The greatest interest in the observation lies in the fact that fairly accurate determinations of stellar velocities are shown to be possible down to the eighth or ninth photographic magnitudes, provided the spectra contain well defined lines.

The importance of permanent adjustment of this spectrograph was realized from the first, but the use of nearly all its parts in the Mills spectrograph necessitated a practical reconstruction of the single prism instrument whenever it was used. The Mills spectrograph was recently rebuilt, and all the parts of Spectrograph I were made permanently available, with the exception of the collimator lens, the slit-head, and the comparison-spectrum apparatus. Professor Camp-

bell asked me to design these parts in order to complete the instrument.[1] A slit-head[2] was adapted from Keeler's star spectroscope, and the comparison apparatus was patterned closely after the device proposed by Wright.[3] The new collimator lens was essentially a counterpart of the old one. The focal plane of the camera lens was found to be nearly flat. No wedge was required under the plate-holder with $H\gamma$ light at minimum deviation, and excellent definition for velocity work was secured from λ 3800 to λ 4600.

A brief description of Spectrograph I appears in *Bulletin* No. 8 of the Lick Observatory. A more detailed description of some of the parts is given in Director Campbell's article on the Mills spectrograph; but for the sake of completeness the principal constants will be repeated here.

Focal length of collimator - - - - - -	720 mm
Aperture of collimator - - - - - - -	37 mm
Refracting angle of prism - - - - - -	60°
Focal length of camera lens - - - - -	406 mm
Dispersion per t.-m. at $H\gamma$, - - - - -	8″

The performance of this spectrograph is exceptionally good. The limit of resolving power with lantern-slide plates is about 0.50 tenth-meters at $H\gamma$, which is all that could be expected of an instrument of one-fifth the dispersion of the Mills. However, with rapid emulsions, the increased size of silver grains interferes seriously with the definition. Careful tests by Dr. J. Stebbins and Dr. J. H. Moore have shown that all effects of flexure have been successfully eliminated in the construction of this instrument. But the effect of temperature-change on the position of lines is very marked. This is largely due to the unequal expansion in the triangle formed by the steel collimator tube, the brass camera tube, and the brass tie-rods which extend from the collimator tube nearly to the plate-holder. The extent of this shift has been determined by Dr. Moore. It is equivalent to approximately 36 kilometers in velocity per degree Centigrade change

[1] All these improvements, as well as a constant-temperature case and thermostat, have been supplied by a grant from the Draper Fund of the National Academy of Sciences.—W. W. C.

[2] Described in *Publications of the Lick Observatory*, 3, 174.

[3] ASTROPHYSICAL JOURNAL, 12, 274, 1900.

in temperature. The limitations of the instrument are thus readily appreciated. In my own work, in the absence of a temperature case, the spectrograph has been wrapped in several thicknesses of woolen blanket just before the exposure began. At the same time all windows in the dome were closed. These precautions, together with the frequent introduction of the comparison spectrum, have made good results possible, but it has been almost invariably the case that the best velocity determinations have been made from plates for which the temperature range was a minimum. Recently a temperature case and automatic thermostat have been constructed for this instrument. These may be expected to result in very material improvement in its performance.

Further disadvantages are inherent in an instrument of low dispersion and low resolving power. Linear defects in lines produce five times the error that they would occasion with the Mills spectrograph. Probably no more than fifteen or twenty single lines could be measured with any power on any one plate of a solar-type star. Stebbins found only six coincidences of solar and stellar lines for *o Ceti* in a region of 340 tenth-meters, while in a region covering 120 tenth-meters he found twenty such lines on Mills plates. With a power of ten, absolutely no single lines would be available for velocity determinations. In view of these facts, it would seem that few tests could be more exacting than the application of the velocity-standard method to this case. Spectrograms made with an instrument of this kind with no greater protection against temperature-change are affected by errors which accurate measurements serve to bring out; but it was one aim of the author to determine the degree of accuracy to be expected from an instrument of low dispersion, for its field of usefulness is practically inexhaustible, as it reduces the exposure time required with the Mills spectrograph by 90 per cent.

(*b*) *The fundamental velocity tables.*—Following the plan of the method above described, Zero-Standard Tables II and III have been prepared from three sky and iron plates with a Toepfer measuring engine, whose least reading is 0.00025 mm. These tables will be directly useful only to myself in the reduction of plates made last year, but will serve as a complete illustration of the method, if not as

guides to other observers in the choice of lines. Corrections for radial velocity and diurnal rotation of the Earth have not been applied, as they were considered negligible. The fundamental dispersion was obtained from a star plate at the mean temperature for the series to be measured. To this dispersion all the plates have been separately reduced. The results for all lines of wave-length greater than λ 4400 are inferior, for the reason that beyond this point the sensitiveness of the lantern-slide emulsion drops suddenly, and the character of the iron lines suffers marked decline; but it has been considered wise to include them in the measures, as the three comparison lines at λ 4900 afford a good control for the curve.

Wave-lengths of all the lines sufficiently accurate for purposes of identification appear in the first column of both tables. The second column contains the factors to convert micrometer displacements into kilometers. The quantity, V_s, which enters into these factors, was taken directly from Frost's "Scheiner's Astronomical Spectroscopy."

Columns 3 and 4 of Table II contain the micrometer readings on the star and iron lines respectively. The number of measures on which each reading depends is given in column 6. In addition to the three standard plates, five other star plates were included in the determination of the iron lines.

All measures for Table II were made with a magnifying power of twenty-five, while Table III was prepared for a power of ten or twelve. In forming the original low-power tables, the measures of two standard plates with a power of ten were separately reduced to the fundamental dispersion by comparison with the high-power table of comparison lines, which could then be adopted for the comparison-line readings of the low-power tables. On comparison of this table with measures of star plates some discrepancies were found. These would arise from actual errors in the table or a difference in the wave-lengths of the lines in Sun and star. Several measures in Table II which came into agreement with the low-power star measures were then introduced into the low-power table. Seventeen of the best of the remaining new or discordant lines in the star were determined from thirty plates and are included in Table IV.

In the measurement of thirty plates of *W Sagittarii* made in 1903, the one aim was to obtain the best possible relative velocities without any sacrifice of absolute determinations. As the mean error in the setting of any line in the original table was found to be equivalent to about 2.3 km displacement, it was conceivable that a velocity determination from the lines measurable on any one plate would differ slightly from a corresponding determination from any other plate, due to errors in the fundamental table. To eliminate this possibility a rather laborious method of reduction was employed. In the first place, all the plates were reduced by the original table. The residual for each line was then formed for every plate, and the mean of all the residuals for any line was applied as a correction to the reading for that line in the original table. The weighted mean of the readings for any line occurring in the original table and in the table thus prepared from thirty plates was adopted as the final setting for that line. These means for all the lines appear in column 3 of Table III. The relative accuracy of these values thus depends upon measures of thirty-three plates, while their absolute accuracy depends upon the three original standard plates. With this new table, all the plates were reduced again.

As a further move toward greater relative accuracy, the seventeen additional star lines which were determined from thirty plates were used with the original standard lines in the reductions. In Table IV the results for these seventeen lines are given under headings similar to those in Table III.

The settings for the iron lines in column 4 of Table III depend upon low-power measures of thirty-one plates which were reduced to the fundamental dispersion, and averaged. Column 5 contains the probable errors of the quantities appearing in the two preceding columns as determined from the observations on star plates only. The number of these observations is given in column 6.

In column 5 of Table II and column 7 of Table III a brief description of each line is given. The character of the line is indicated by the letters vG (very good), G (good), F (fair), and P (poor). Then follows the intensity according to Rowland's scale. The remaining comments are self-explanatory, but include the following arbitrary

abbreviations: *b*, broad; L, line; Gr, green; V, violet; inc, included; shp, sharp; gp, group; B, bright; Cl, close; I, intensity; and F, faint.

Column 8 of Table III contains the differences between the settings for the various lines in the original solar table and the corresponding values in the final table in column 3. Regarding these as residuals for the measures of the original table, the probable error of the absolute velocities as affected by the errors in the original low-power table is seen to be ± 0.24 km.

No correction for curvature has been introduced into either of these tables. It should be noticed that this correction is entirely differential in its nature with this method of measurement and is negligible if the slit-length is not altered. The formulæ for the computation of this small correction, based on the assumption of a parabolic curvature of the spectral lines, are as follows:

$$\text{For Table II,}\quad dV = -(y^2 - 1.2)\ 0.5 \text{ km},$$
$$\text{For Table III,}\quad dV = -(y^2 - 1.1)\ 0.46 \text{ km},$$

where y is the distance in units of $\frac{1}{4}$ millimeter from the center of the spectrum to the measured part of the comparison line.

TABLE II.

Standard Table (Magnifying Power 25).

Wave-Lengths	rV_s	Micrometer Readings: Sky	Micrometer Readings: Iron	Notes	Observations
3969.4			13.879	G λ3969.413	7
3982.0	760	14.928		F – G 10*b*	3
3987.0	767	15.634		G 20	3
3994.7			16.668	F	3
4005.3			17.421	G 4005.370	8
4005.3	778	17.423		G 20	3
4006.8	780	17.562		F 15*b*	3
4012.5	781	18.100		F 10*b*. Shp. Ls to V	3
4013.9	782	18.252		F 10*b*	3
4014.2	782	18.288		F 20 Mean.	3
4014.6	783	18.338		P 8	3
4018.0	784	18.625		F 18	3
4021.8			19.009	F	2
4022.0	787	19.023		G 10*b*. LF6 at –.070	3
4025.0	789	19.301		F 15. f Ls to V.	3
4028.6	791	19.634		F – P 8. *b*.	2

TABLE II—*Continued.*

Wave-Lengths	rV_s	Micrometer Readings		Notes	Obser-vations
		Sky	Iron		
4029.8	792	19.748		F 6	2
4030.9	792	19.845		G 15	3
4030.9			19.847	F–G	8
4032.0	793	19.978		P 8	2
4033.0	793	20.057		F–G 10*b*	3
4034.6	794	20.201		G 9	3
4035.8	795	20.313		G 8	2
4040.2	797	20.711		F 5	2
4040.9	798	20.778		F 7	2
4041.7	800	20.853		F 8*b*	3
4044.0	801	21.083		G 8	3
4044.8	801	21.162		F 6	3
4046.0			21.249	G 4045.975	8
4046.0	802	21.252		G 20	3
4048.8	804	21.550		F 7	3
4052.4	806	21.871		F 18*b*	2
4055.3	807	22.090		F 18*b*	3
4057.6	809	22.330		G 10	3
4063.7			22.878	G 4063.705	8
4063.9	812	22.882		G 17. L at .075 not inc.	3
4071.9			23.613	G 4071.895	8
4072.0	818	23.615		G 15. L at .075	3
4077.9	822	24.174		G 10	3
4078.6	822	24.252		P 6*b*	3
4079.4	823	24.313		F 8*b*. F Ls to Gr	3
4092.7	832	25.463		G 12	3
4096.2	836	25.787		F 8	3
4098.5	836	25.978		F 10	3
4100.2	838	26.115		F 7. Mean 2 cl. Ls.	3
4104.4	840	26.524		F 6. LF 8*b* at +.060	3
4106.5	842	26.668		F 5.	3
4107.6			26.777	F	4
4107.7	842	26.782		F 7	2
4115.0	847	27.418		F 12*b*	3
4116.7	848	27.561		F 10*b*	2
4118.8			27.730	G 4118.806	8
4118.9	850	27.741		G 12	3
4121.8	851	27.997		F–P 10 Mean 2 Ls	3
4123.9	853	28.163		F 8*b*	3
4126.0	854	28.339		G 7	3
4126.2	854	28.360		G 15*b*. Mean	3
4126.5	855	28.392		G 7	3
4128.0	856	28.518		F 12*b*.	3
4132.5			28.881	G	8
4132.6	859	28.892		G 15	3
4134.0	859	29.043		F 8	3
4134.5	860	29.077		G 18	3
4134.7	860	29.101		F 10*b*	3
4143.9	867	29.865		G 15	3
4144.0			29.866	G 4143.955	7
4150.0	871	30.358		P 20	3

TABLE II—*Continued.*

Wave-Lengths	rV_s	Micrometer Readings		Notes	Observations
		Sky	Iron		
4152.3	872	30.553		G 10	3
4154.1	873	30.715		F 7	3
4154.4	873	30.741		G 15*b* Mean.	3
4154.6			30.739	F 4154.571	6
4154.8	874	30.771		F 7	3
4156.5	874	30.893		F 8	3
4156.8	875	30.947		G 15	3
4157.0	875	30.923		G 8	3
4157.9	876	31.044		P 8	2
4160.7	878	31.275		F 8*b*	3
4161.2	878	31.310		G 12	3
4161.5	878	31.336		F 8*b*	3
4163.8	879	31.539		F 8	3
4165.7	880	31.654		F 8*b*	3
4167.4	882	31.819		vG 15	3
4171.2	884	32.118		F 7. Shp.	2
4172.9	886	32.264		F 7	3
4175.1	887	32.438		F 6. Shp.	3
4177.8	888	32.652		F 15. Mean.	3
4182.2			32.985	G. 4182.155	8
4182.3	891	32.988		G 10. L.F5 at +.067	3
4187.3	894	33.414		G 8	3
4187.6	894	33.433		G 17. Inc. L 6.4186.9	3
4187.6			33.438	F–G 4187.572	8
4187.6	894	33.438		G 15. Mean 4187.3 +4187.9	2
4187.9	895	33.465		G 8	3
4188.9	896	33.562		G 5	3
4191.7			33.746	F 4191.678	6
4191.6	897	33.766		G 10*b* L.P6+.080 not inc.	3
4198.5	901	34.317		G 10	3
4198.6	901	34.326		G 16 4198.5+4199.2	3
4198.7			34.351	G	8
4202.2			34.594	G 4202.185	8
4202.2	903	34.603		G 10 L.F7 at −0.080	3
4207.0	906	34.992		F 9*b*.	3
4216.0	912	35.676		G 20	3
4219.5			35.955	F	8
4219.5	914	35.956		F 8	3
4222.3			36.174	F	5
4222.3	916	36.175		P 7. cl.Ls.	2
4226.9	919	36.524		F 15 "g"	3
4227.6			36.578	G	8
4227.6	919	36.580		F 9	3
4229.8	920	36.757		G 10	3
4233.5	923	37.031		G 15	3
4233.6			37.040	G–F	8
4236.1	925	37.228		G 12	3
4236.1			37.230	G. 4236.11	8
4243.0	930	37.765		G 30. Shp. edges	3
4250.4	936	38.307		G 8	3

TABLE II—*Continued.*

Wave-Lengths	rV_s	Micrometer Readings		Notes	Obser-vations
		Sky	Iron		
4250.8	936	38.337		G 17 Mean.	3
4250.6			38.343	G 4250.62	8
4251.1	936	38.368		G 8	2
4254.4	939	38.635		G 10	3
4260.5			39.083	G 4260.50	8
4260.4	942	39.084		G 12.	3
4271.7	950	39.910		G 18	3
4271.8			39.916	G 4271.76	8
4275.2	952	40.152		F 25, 0.120−0.180	2
4280.5	957	40.541		F 15. Close Gp.	3
4282.6			40.700	G. 4282.56	8
4283.0	959	40.709		G 10*b*	3
4289.7	962	41.229		F 20, 0.180−0.270	3
4294.3			41.555	G 4294.30	8
4294.2	967	41.567		F 8	3
4297.0	968	41.765		G 12. 0.730−0.800	3
4299.4			41.924	G 4299.41	8
4299.6	969	41.939		F 20*b*	3
4301.7	970	42.035		F 7	2
4308.1			42.546	G 4308.08	8
4308.1	976	42.553		F 15	3
4314.1	980	43.002		F−P 7	3
4314.5	980	43.033		G 20. Gp. 0.970	3
				−1.090	3
4315.2	981	43.062		F−P 7	3
4318.8	983	43.301		G 10	2
4321.0	984	43.440		F 10	2
4321.9	985	43.497		F 5	3
4323.6	987	43.654		G 20	3
4325.3	988	43.776		F 80	3
4325.8	989	43.808	43.811	G 20	3
4326.4			43.811	G 4326.40	8
4326.6	990	43.828		F 8	3
4331.2	992	44.159		F 10*b* Gp.	3
4337.2	997	44.608		F 9*b*	3
4337.6	998	44.634		F 18	3
4338.0	998	44.657		F 8	3
4340.7	999	44.835		G 20 "$H\gamma$."	3
4344.1	1002	45.093		F 12*b*	3
4346.7	1003	45.254		G 8	3
4352.0	1007	45.611		G 8	3
4352.3	1007	45.641		G 18 0.600−0.685	3
4367.8	1019	46.711		F 10 Shp.	3
4370.0	1021	46.844		F−P 8	3
4371.2	1022	46.930		F 8	2
4374.7	1024	47.144		F 8*b*	3
4375.4	1024	47.189		F 18	3
4376.1	1025	47.230		F 8	2
4383.7	1031	47.758		G 25	3
4383.7			47.764	G 4383.67	8
4385.2	1031	47.866		F 9	3
4399.9	1041	48.844		F 6	3

TABLE II—*Continued.*

Wave-Lengths	rV_s	Micrometer Readings		Notes	Obser-vations
		Sky	Iron		
4400.6	1042	48.887		F 10*b*	3
4408.8	1042	48.904		G 25	3
4401.5	1043	48.939		G 8	3
4403.3	1044	49.060		G−F 7	3
4404.9	1045	49.163		G 12	3
4404.9			49.164	G 4404.93	8
4415.3			49.836	G−F 4415.29	8
4415.4	1052	49.841		G 15	3
4422.8	1059	50.303		F 10*b* 0.245−0.320	3
4427.5	1062	50.620		G 8*b*	3
4430.8	1064	50.823		F−G 15	3
4435.5	1068	51.140		F−P 20	3
4447.5	1077	51.908		G 15.	3
4451.2	1079	52.093		F 12	3
4453.3	1081	52.267		F 5	3
4455.1	1082	52.381		G 12	3
4458.4			52.650	G−F	7
4462.0	1088	52.808		G 12	3
4466.7	1092	53.105		F 7*b*	2
4466.7			53.106	F−G 4466.73	8
4476.2			53.688	G 4476.18	8
4482.4	1102	54.060		F 8	2
4482.4			54.063	F−G 4482.39	8
4494.7			54.814	G 4494.74	8
4494.8	1112	54.815		F 10*b*	3
4501.4	1118	55.230		F 20	3
4518.0	1130	56.209		F 8*b*	3
4520.4	1132	56.342		F 7	3
4522.9	1134	56.495		G 8	3
4525.3	1136	56.642		F 6	2
4526.8	1137	56.735		F 8	3
4528.9			56.846	F−G	8
4529.0	1138	56.852		F 8*b*	3
4531.3	1140	56.987		G 9	3
4545.0	1151	57.795		F 8*b*	3
4549.8			58.046	F	8
4549.8	1153	58.048		G 10*b*	3
4552.8	1155	58.228		F 8	2
4554.3	1156	58.317		F 8*b*	2
4556.2	1158	58.436		G 12*b*	3
4563.9	1162	58.866		F 6	3
4571.8	1169	59.333		F 15	3
4584.0			60.009	F−G. 4584.02	8
4590.2	1183	60.340		F 8	3
4598.1	1188	60.778		F 8*b*	3
4611.3	1200	61.656		G 7	3
4630.—			62.580	F	8
4871.9			73.896	G 4871.88	5
4859.9			74.430	F 48.5993	2
4891.4			75.301	G 4891.37	5
4920.7			76.560	F 4920.68	1
4957.7			78.156	G 4957.67	5

TABLE III.

Standard Table (Magnifying Power 10–15).

Wave-Lengths	rV_s	Micrometer: Sky and Star	Micrometer: Iron	P. E.	Observations	Remarks	Reduction from Sky Plates
3969.4			13.8790	±0.0003	..		+0.001
4005.3			17.4189	0.0002	..	G	+0.003
4005.3	780	17.424*		0.0008	26	G 25. Close BL to Gr.	+0.001
4012.5	780	18.104*		0.0010	25	F 13. 0.080−0.150. Cl Ls to V.	+0.004
4024.8	790	19.289		0.0016	19	G 30*b*	+0.003
4041.0	800	20.795		0.0014	24	F 25. 0.740−0.840=3Ls.	+0.002
4046.0			21.2472	0.0003	..		
4045.9	800	21.252*		0.0009	27	G 20. Cl. Ls to V.	−0.005
4063.7			22.8785	0.0003	..		
4063.9	810	22.881*		0.0009	20	F−G17.	−0.001
4066.8	810	23.187		0.0012	18	G15.	+0.002
4067.2	810	23.211		0.0010	18	F 30 Mean.	+0.001
4072.0	820	23.612*		0.0009	22	G 20.	−0.007
4071.9			23.6127	0.0003	27		
4092.8	830	25.462*		0.0009	14	G 12. L F12 at −0.160.	−0.001
4101.7	840	26.270		0.0011	25	G 30. 0.220−0.350. $H\delta$.	0.000
4118.8			27.733		2		
4118.9	850	27.747*		0.0009	23	G12 L+0.100 F6.	−0.004
4128.0	860	28.523		0.0015	16	F12*b*. Ls F15 at −0.150 and at +0.144	+0.002
4132.5			28.8831	0.0003	28		
4134.5	860	29.086		0.0016	17	G20	+0.001
4137.0	860	29.315		0.0013	20	G15. In *b* region.	−0.003
4143.6	870	29.840		0.0010	9	G25. Weight I. L.F12 at −0.142 not inc.	−0.001
4143.9	870	29.863		0.0013	25	G 12	−0.003
4144.0			29.8655	0.0002	28		
4150.0	870	30.364*		0.0011	25	G−F20	−0.002
4152.3	870	30.556		0.0013	18	G 10	0.000
4154.4	870	30.741*		0.0009	28	G 15	+0.002
4156.8	870	30.923*		0.0008	28	G 15	−0.002
4061.2	880	31.319*		0.0000	28	G 12	+0.005
4163.6	880	31.521*		0.0008	20	G 11	0.000
4178.5	890	32.712*		0.0008	28	G 40. L+0.130 not inc.	+0.004
4182.2			32.985		8		
4182.3	890	32.995*		0.0009	22	G 10	0.000
4184.7	890	33.178		0.0016	17	F 20. Mean of 2Ls.	+0.003
4187.6	890	33.434*		0.0010	27	G 17. LG 5+0.125 not inc.	−0.003
4191.7	900	33.771		0.0018	12	G−F10.	−0.001
4196.0	900	34.109		0.0013	26	G−F25. Mean of 2Ls.	+0.004
4198.5	900	34.325*		0.0009	22	G 20.	+0.004
4198.6	900	34.339		0.0013	5	G 25. Inc.L.5 4199.3	−0.001
4198.7			34.3524	0.0003	28		
4202.2			34.5927	0.0004	28		
4202.2	900	34.603		0.0011	22	F 25	+0.001
4216.0	910	35.673*		0.0009	28	G 25. L 4217.7 A.U. not inc.	0.000
4227.0	920	36.539		0.0011	23	F−G30.	+0.001
4227.6			36.584		6		
4233.5	920	37.028*		0.0008	28	G−F15.	−0.005
4233.6			37.042		8		
4236.1			37.233		9		
4236.2	920	37.234		0.0011	16	F20. Whole L. Weight 1.	−0.004
4236.3	930	37.252		0.0014	9	Middle of L.	0.000
4250.6	940	38.341*		0.0013	25	G 17	0.000
4250.6			38.3417	0.0003	27		
4260.5			39.0834	0.0003	28		
4260.4	940	39.089*		0.0011	25	G 12. LF10 at −0.140	+0.001
4271.7	950	39.911		0.0014	29	G 20	−0.003
4271.8			39.9130	0.0003	29		
4275.1	950	40.154*		0.0007	18	F−G25 0.120−0.180	0.000
4282.6			40.6989	0.0003	20		
4283.2	960	40.721*		0.0011	24	F10*b*.	0.000
4290.0	960	41.231		0.0011	6	F20 0.180−0.270	+0.002
4204.3			41.5562	0.0004	28		
4294.3	970	41.562		0.0015	13	F10	−0.005
4294.4	970	41.580		0.0022	12	G20. Cl.L to Gr	−0.002
4299.4			41.9266	0.0004	25		

TABLE III—*Continued.*

Wave-Lengths	rV_s	Micrometer		P. E.	Observations	Remarks	Reduction from Sky Plates
		Sky and Star	Iron				
4300.4	970	41.964		0.0011	28	G30 LP18 to Gr not inc. Wide group.	−0.003
4306.2	970	42.382		0.0013	27	F−G10	+0.004
4308.1			42.5472	0.0003	28		
4326.4	990	43.806*		0.0010	27	G20	−0.006
4326.4			43.8096	0.0004	29		
4331.2	990	44.154*		0.0010	27	F−G10	+0.001
4340.7	1000	44.839*		0.0009	27	G20 $H\gamma$	+0.003
4344.1	1000	45.095*		0.0010	29	F15*b*	+0.002
4375.4	1020	47.186*		0.0008	30	G20	0.000
4383.7			47.7651	0.0003	29		
4395.0	1040	48.544*		0.0011	27	G−F15 Ls to V.	−0.004
4401.2	1040	48.895		0.0011	16	Dense part to Gr.	−0.004
4404.9	1050	49.161		0.0013	24	G12 LP8 at −0.070	+0.003
4404.9			49.1636	0.0002	29		
4415.3			49.8347	0.0003	27		
4415.3	1050	49.837*		0.0010	28	GF15	+0.002
4422.8	1060	50.308		0.0012	18	F15*b* 0.246−0.360. LF10+0.100.	+0.001
4443.0	1070	51.647*		0.0010	28	F25 0.590−0.720.	0.000
4451.3	1080	52.102*		0.0011	21	G18. G12 in V. Measure whole line.	−0.001
4455.4	1080	52.397		0.0014	22	F25 Mean of 2Ls.	+0.003
4462.0	1090	52.806		0.0014	24	F12	+0.001
4466.7			53.1055		10		
4476.2			53.6887	0.0007	21		
4482.0	1100	54.002		0.0015	22	F25	−0.002
4494.7	1110	54.806		0.0024	12	F10*b*	+0.002
4494.7			54.8121	0.0007	25		
4501.3	1120	55.220		0.0011	22	F20. Measure sharp middle.	−0.005
4528.9			56.8404	0.0014	7		
4541.0	1150	57.552		0.0017	9	F15*b*. Ls to V not inc.	−0.002
4540.8	1150	58.056*		0.0010	23	G20	+0.003
4571.8	1170	59.332*		0.0009	24	F15	+0.005
4584.0			60.0115	±0.0008	23		
4630			62.5809		16		
4871.9			74.4299		16		
4891.4			75.3012		17		
4957.7			78.1556		17		

TABLE IV.

Additional Lines in Star (Low Power).

Wave-Lengths	rV_s	R	P. E.	Obs.	Remarks
4063.6	810	22.860	±0.0011	8	F25 Wt. I.
4132.7	860	28.905	0.0012	27	G15
4137.3	860	29.338	0.0009	18	25 whole line
4191.4	900	33.751	0.0013	20	GF20. L to V. included.
4204.7	900	34.834	0.0016	20	F18. LF12 at +0.120.
4290.1	960	41.257	0.0010	27	F30 Weight I. LP12 at −0.140.
4297.0	970	41.745	0.0015	11	F−G12 0.700−0.800
4314.4	980	43.026*	0.0009	25	G20 0.970−1.090
4334.7	990	44.372	0.0014	17	F−G18. L at +0.100 not inc.
4337.8	1000	44.648*	0.0010	27	F−G25
4352.0	1010	45.611*	0.0011	29	G30 L at 0.150 not inc.
4360.0	1010	46.113	0.0011	24	F−G20
4384.5	1030	47.814*	0.0009	29	G38
4401.0	1040	48.883=	0.0006	19	G25 L at −0.170 not inc.
4417.9	1050	50.002	0.0014	17	F20 Mean of 2Ls.
4466.4	1090	52.974	0.0017	12	F10
4583.7	1180	59.988	±0.0011	17	F−G20

(*c*) *Application to the study of the variable star, W Sagittarii.*—Exhaustive studies of the light curve of *W Sagittarii* were made by J. H. F. Schmidt, who discovered the variability of this star, then known as γ' *Sagittarii*, at Athens in 1866.[1] From a series of 890 observations covering ten years and extending over 195 maxima and 193 minima he constructed a light-curve, which is drawn in detail in Fig. 1 of this paper. In addition to the strong irregularities in the curve, he suspected a perturbation in the light-period running through a cycle of eight years, affecting the time of maximum and minimum by several tenths of a day. But his observations were hardly extensive enough to warrant this last conclusion. The spectrographic observations at present available do not cover a period long enough to confirm or to disprove this result. As presented in the Chandler and Harvard Catalogues, the important data regarding this star, with some additions, are as follows:

CHANDLER'S THIRD CATALOGUE.

Max., $4^{m}.8$; Min., $5^{m}.8$; M–m, $3^{d}.00$; Period, $7^{d}.59460$ E.
Epoch of Max., 1866 Sept. 4; Julian, $2402849^{d}.45$.

HARVARD CATALOGUES.

Max., $4^{m}.3$; Min., $5^{m}.1$; Class IV; Sp., G5K.
α, 1900.0, $17^{h}\ 58^{m}.6$; δ, 1900.0, $-29^{\circ}\ 35'$.

ADDITIONAL DATA.

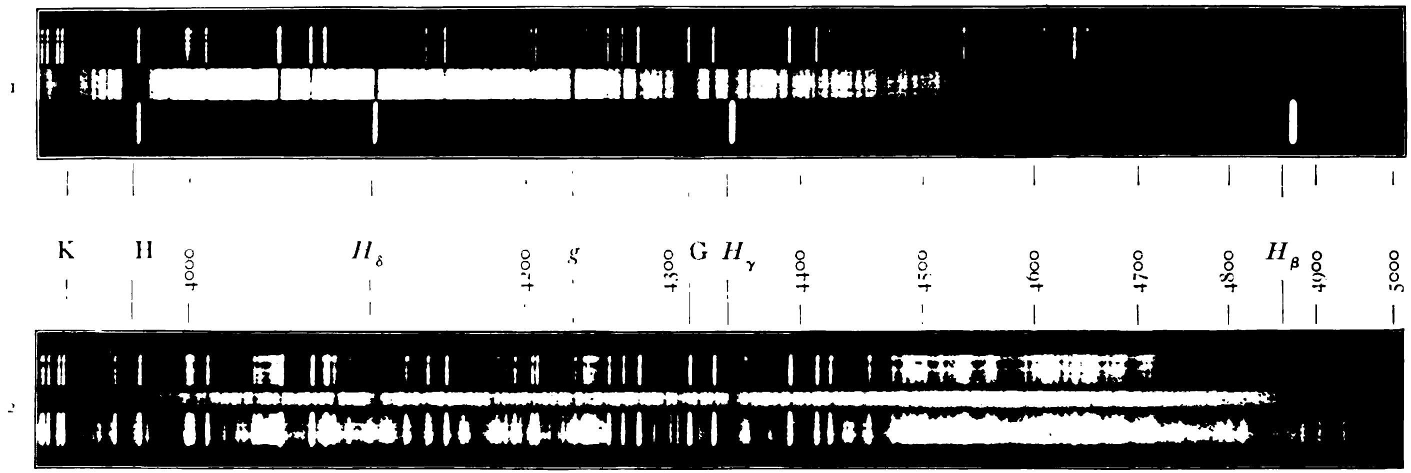
λ, 1902.0, $269^{\circ}\ 44'$; β, 1902.0, $-6^{\circ}\ 8'$
Photo. Max., $5^{m}.5$; Photo. Min., $6^{m}.5$.

The spectrum *W Sagittarii* approximates very closely to the solar type. The fact that sixty-five blends ranging in intensity from 12 to 40 gave good results, as exhibited in the last column of Table III, establishes the close resemblance of this star to our own central body. The seventeen lines in Table IV suggest some differences, the most striking of which is the strong line at λ 4334.7, which is not found in the Sun, but rises to an intensity of 12 in the star. But, on the whole, the identities are far-reaching enough to warrant an extensive system of blending.

This fact justified the use of a power of ten in the measures of the

[1] *Astronomische Nachrichten*, **87**, 103, 1876.

PLATE XIII.



1. Spectra of { Iron. / Sky. / Hydrogen.

2. Spectrum of *W Sagitarii*, 1903, September 16. 77A.

plates of *W Sagittarii*. But the selection of such a low power was not decided upon without considerable experiment with a higher one. With power 10, the width of the micrometer thread (about 0.50 tenth-meters) interfered with settings on lines of intensity 12 to 15, while the density of the comparison lines was so great that it became difficult at times to distinguish the black vertical wire when set on them. After experimenting with powers of 10, 12, 15, and 25, I am inclined to think that a power in the neighborhood of 15 would perform most satisfactorily in the case of solar-type stars.

From preliminary measures in August 1903 of three spectrograms of *W Sagittarii*, a very considerable variation in the radial velocity of this star was brought to light. Subsequently a series of thirty-three spectrograms, distributed uniformly in the light-period, were secured with Spectrograph I. These have been measured and reduced for the purpose of determining the character of the orbit of this interesting variable. I have not been able to recognize upon these plates any effect attributable to the light of the fainter companion now known to exist, but it should be remembered that a difference of one magnitude is nearly sufficient to obliterate the effect of the darker star.

The details of the production of these plates are recorded in the *Journal of Observations*, Table V. The longer exposures of the comparison were made with a diaphragm occulting all lines to the violet of λ 4415, while during the shorter exposures the entire plate was uncovered. When the comparison was introduced four times, it was photographed once with the diaphragm and once without on each side. When introduced six times, four exposures were made without and two with the occulting device. With ten introductions of the comparison, three exposures were made on each side with the diaphragm removed, and two on each side with the diaphragm in position. The slit-width is expressed in terms of the divisions of the slit micrometer-head. The unit is 0.025 mm. To illustrate better the methods employed, complete measures and reductions of Plate 56A are published in Table VII. The columns from left to right contain the wave-lengths of the spectral lines, the micrometer readings on the star, those on the comparison, the reduction to fundamental dispersion, the displacements of the star lines from the solar lines of the

zero-standard table, and the equivalents of these displacements in kilometers per second. Referring to the description of the plates in the last two columns in Table VI, and to the temperature-range in Table V, it is evident that 56A is about an average plate.

TABLE V.

Journal of Observations (*W Sagittarii*).

Plate	Astronomical Date	Mt. Hamilton Sidereal Time, Mid-Exposure	Duration	Comparison		Observing Room Temperature			Slit-Width	Seeing	Remarks
				Times	Length of Exposure	Begin C.	End C.	Range C.			
	1903	h m	m		s s	°	°	°	in.		
20B	Aug. 3	17 47	45	4	5 &25	18.3	18.2	0.1	1.4	Fair	
25A	9	17 40	40	4	4 &20–30	24.5	24.2	0.3	1.3	Good	
36D	Aug. 17	17 52	45	4	5 &25–30	20.8	20.7	0.1	1.4+	Poor	
46A	Sept. 2	18 21	30	6	3 &25	24.8	24.4	0.4	1.3	Good	
47B	4	18 38 ± 3m	35 ± 5m	6	3 &27	....	12.8	....	1.4	Very Poor	Wind 30 miles.
56A	7	18 9	45	6	3 &26	17.2	16.4	0.8	1.3+	Fair	
57B	7	18 49	28	6	3 &26	16.4	16.3	0.1	1.3+	Fair	
58A	8	18 9	25	6	3 &26	19.7	19.2	0.5	1.4	Fair	
59D	8	18 38	30	6	3 &26	19.2	18.8	0.4	1.4	Good	
60A	9	18 9	15	6	2.5 &26	21.7	21.4	0.3	1.3	Good	
61D	9	18 32	24	6	2.5 &26	21.4	....	0.3	1.3	Fair	
63A	10	18 7	20	6	2.5 &26	21.4	....	0.5	1.3	Fair	
64B	10	18 28	19	6	2.5 &26	....	20.5	0.4	1.3	Fair	
65A	11	18 4	15	6	3 &26	16.0	....	0.4	1.3	Fair	
66B	11	18 23	20	6	3 &26	....	15.3	0.3	1.3	Fair	
75A	14	18 33	35	6	2–3 &25	13.6	12.8	0.8	1.3	Fair	
76B	14	19 1	21	6	2–3 &20	12.8	12.6	0.2	1.3	Good	
77A	16	18 25	24	6	25	19.2	18.5	0.7	1.3+	Good	
78B	16	18 54	32	6	2–3 &20	18.5	18.0	0.5	1.3+	Good	
82A	20	19 1	83	6	2–3 &25	18.9	17.5	1.4	1.3+	Good	Clouds cut off ½ light.
83A	21	19 15	85	6	3–4 &23	18.4	17.8	0.6	.....	Poor.	Mid-comparison at 19h.om. Smoky sky cuts off ½ light.
86A	23	19 11	65	6	3 &25	19.4	19.3	0.1	1.3+	Bad	
91A	24	18 45	30	6	3 &25	22.5	21.8	0.7	1.3+	Fair	
92B	25	18 56	43	6	3 &25	19.1	18.7	0.4	1.3+	Good	Smoke reduces image ½
93B	26	18 51	35	6	....	19.8	19.8	0.0	1.3	Good	Short circuit in comparison Gave extra time.
94D	27	19 3	53	6	3 &25	16.7	16.6	0.1	1.3+	Poor.	
95B	Oct. 3	19 24	25	6	3–6 &25–40	11.0	10.4	0.6	1.3+	Fair	
99F	4	19 46	76	6	3–4 &26	11.7	11.0	0.7	1.3	Good to Bad	Image faint Objective probably fogged.
100A	5	19 47	45	6	3.5–4 &25	11.8	11.3	0.5	1.3	Poor to Fair	
102A	6	19 36	39	6	3–4 &25–35	14.6	14.2	0.4	1.3	Good	
103B	6	20 13	33	6	3–4 &25–35	14.2	13.8	0.4	1.3	Good to Poor	
	1904										
136F	June 7	17 16	52	10	1.5 &11	14.6	14.0	0.6	1.3+	Poor.	
137A	7	18 1	31	10	1.5 &11	14.0	14.0	0.0	1.3+	Fair	

TABLE VII.

Measures and Reduction of Plate 56A *W Sagittarii*.

λ	Star	Iron	Red.	Dis.	V	λ	Star	Iron	Red.	Dis.	V
					km						km
3969.4		13.876				71.7	39.945		−3	31	29
4005.3		17.418		+	+	71.8		39.9195			
05.3	17.455		+1	32	25	75.1	40.177		−3	20	19
12.5	18.136			33	26	82.6		40.6995			
24.8	19.316			28	22	83.2	40.753		−2	30	29
41.0	20.839			45	36	90.0	41.257		−2	24	23
46.0		21.246				4290.1	41.285		−2	26	25
45.9	21.273		+1	22	18	94.3		41.5555			
63.7		22.879				94.4	41.608			26	25
63.6	22.881		±0	21	17	99.4		41.930			
67.2	23.233			22	18	4300.4	41.986			20	19
72.0	23.635			23	19	06.2	42.400			16	16
71.9		23.6125				08.1		42.5515			
4101.7	26.287			17	14	14.4	43.048			20	20
18.9	27.773			26	22	26.4	43.823			15	15
28.0	28.558			35	30	26.4		43.808			
32.5		28.882				31.2	44.180			24	24
32.7	28.935			30	26	34.7	44.395			21	21
34.5	29.112			26	22	37.8	44.680			30	30
37.0	29.325			10	9	40.7	44.869			28	28
43.6	29.861			21	18	44.1	45.110			13	13
43.9	29.896			33	29	52.0	45.639		−2	26	26
44.0		29.8665				60.0	46.130		−3	14	14
50.0	30.399			35	30	75.4	47.213			24	24
52.3	30.582			26	23	83.7		47.7665			
54.4	30.763			22	19	84.5	47.840		−3	23	24
56.8	30.955			32	28	95.0	48.567		−4	19	20
61.2	31.338			19	17	4401.2	48.912			13	14
63.6	31.547			26	23	04.9	49.188		−4	23	24
78.5	32.746			34	30	04.9		49.170			
84.7	33.198			20	18	15.3		49.840			
87.6	33.463			29	26	15.3	49.870		−5	28	29
91.7	33.790			18	16	17.9	50.037			30	31
96.0	34.143			34	31	43.0	51.673		−5	21	22
98.5	34.351			26	23	55.4	52.422		−6	19	21
98.7		34.3525				62.0	52.845		−6	33	36
4202.2		34.592				76.2		53.699			
02.2	34.630			27	24	94.7		54.815			
04.7	34.852			18	16	4501.3	55.240		−7	13	15
16.0	35.694		±0	21	19	71.8	59.350		−4	14	16
27.0	36.558		−1	18	17	84.0		60.015			
33.5	37.047		−2	17	16	4630.−		62.588	Mean	+	22.1km
50.6	38.363		−3	19	18	4871.9		74.436			
50.6		38.343				4891.4		75.320			
60.5		39.089				4957.7		78.172			
60.4	39.110		−3	18	17						

Table VI includes the results of the measures of the plates whose numbers on the observing list occur in column 1. Under the heading V′ the radial velocities with respect to the observer are given. All

the plates were measured without reversal with low power, in order to make the results strictly comparable with those of the standard plates. Eight of these velocities depend upon two measures, but the probable errors in column 7 depend upon one measure only, unless the results from separate lines were combined before the mean was taken. The probable errors of the first thirty-one plates, excepting 25A, were obtained from a preliminary set of reductions. Corrections for the motion of the observer are combined in column 5. The resulting values of the radial velocity of the star with reference to the Sun appear under the heading V in column 6.

Assuming the identity of the light- and velocity-periods for this star, the quantities in column 3 have been formed from the following data:

Maxima of *W Sagittarii*

1903,	August	2.865
		10.559
	September	2.243
		9.838
		17.432
		25.027
	October	2.621
1904,	June	1.649

Employing the quantities in columns 3 and 6 as abscissæ and ordinates respectively, the plot of velocity-determinations was formed which appears in Fig. 1. Ample verification of the adopted value for the period was found in the close agreement between results obtained from plates covering more than forty maxima of the curve, or an interval of 309 days. This agreement was at once evident in Table VI from a comparison of Plates 86A and 136F. But when the attempt was made to pass an elliptic velocity-curve through these observations, it was found that the plotted points oscillated above and below this curve with a period of 3.8 days, or one-half that of the light-variation. After repeated trials of various ellipses with different values of periastron time, longitude of periastron, eccentricity, maximum positive and negative velocity, and the velocity of the system, I selected the velocity-curve which is drawn in Fig. 1 with the narrower line. (The residuals from this ellipse appear under the head *r′* in Table VI.) In selecting this conic it was assumed

TABLE VI.

Table of Velocity Determinations From 33 Plates of *W Sagittarii.*

No.	Date G. M. T.	Interval Since Max.	V'	Reduction to Sun	V	P. E.	*r'*	*r*	Character of Spectra: Star	Character of Spectra: Comparison
	1903	d	km	km	km	km	km	km		
20B	Aug.. 3.713	0.848	−19.8	−19.3	−39.1	±0.7	+4.9	+0.7	Fair	Fair
25A	9.693	6.828	−21.0	−21.5	−42.5	±1.5	−4.2	±0.0	Fair to poor	Fair
36D.	17.678	7.219	−21.4	−23.9	−45.3	±0.6	−1.5	+0.1	Fair	Fair
46A	Sep.. 2.655	0.412	−10.8	−27.6	−38.4	±0.7	+7.4	+3.7	Fair. Overexposed	Fair
47B	4.661	2.418	−10.6	−28.1	−38.7	±0.6	−5.7	−0.4	Good	Fair
56A	7.633	5.390	+22.1	−28.4	− 6.3	±0.5	+0.6	−0.2	Fair	Good
57B	7.660	5.417	+23.8	−28.4	− 4.6	±0.8	+2.4	+1.8	Fair	Good
58A	8.631	6.388	−2.8	−28.5	−31.3	±0.6	−5.5	±0.0	Good	Good
59D.	8.651	6.408	− 4.3	−28.6	−32.9	±0.4	−5.8	−0.3	Good	Fair
60A	9.628	7.385	−15.7	−28.6	−44.3	±0.7	+0.7	+0.7	Fair to good	Good
61D.	9.643	7.400	−16.3	−28.7	−45.0	±0.7	±0.0	±0.0	Fair to good	Good
63A	10.624	0.786	−12.6	−28.7	−41.3	±0.6	+2.8	−1.4	Good	Good
64B	10.637	0.799	−11.3	−28.7	−40.0	±0.6	+4.3	+0.1	Good	Good
65A	11.620	1.782	− 9.6	−28.9	−38.5	±0.5	+0.4	−0.2	Fair	Good
66B	11.632	1.794	− 9.4	−28.9	−38.3	±0.5	+0.6	−0.4	Good	Good
75A	14.631	4.793	+21.3	−29.2	− 7.9	±0.8	+2.5	−1.4	Fair to poor	Fair to poor
76B	14.651	4.813	+23.9	−29.2	− 5.3	∓1.8	+4.9	+1.0	Underexposed	Good
77A	16.620	6.782	−10.7	−29.3	−40.0	±0.5	−5.8	−0.6	Good	Good
78B	16.640	6.802	−11.1	−29.4	−40.5	±0.5	−6.0	−0.8	Fair	Good
82A	20.634	3.202	+ 3.2	−29.5	−26.3	±0.7	−0.2	+3.1	Fair	Fair
83A	21.640	4.208	+18.9	−29.6	−10.7	±0.5	+5.5	+1.9	Fair	Fair
86A	23.632	6.200	+ 4.6	−29.6	−25.0	±0.6	−2.6	+0.1	Good	Fair
91A	24.612	7.180	−11.9	−29.6	−41.5	±0.5	+2.1	+3.0	Good	Good
92B	25.617	0.590	−15.2	−29.6	−44.8	±0.7	+0.4	−3.7	Fair	Fair
93B	26.611	1.584	−10.0	−29.5	−39.5	±0.7	+0.3	−0.5	Good	Good
94D.	27.617	2.590	− 7.3	−29.5	−36.8	+0.7	−5.3	+0.2	Fair	Fair
95B	Oct. 3.615	0.994	−12.5	−29.2	−41.7	±0.5	+1.6	−2.3	Fair	Poor
99F	4.627	2.006	− 9.7	−29.2	−38.9	±0.7	−2.5	+0.1	Overexposed	Poor
100A	5.625	3.004	− 4.9	−29.1	−34.0	±0.6	−4.7	+0.4	Good	Fair
102A	6.615	3.994	+11.6	−28.8	−17.2	±0.6	+1.1	−1.4	Fair	Fair
103B	6.640	4.019	+12.4	−28.9	−16.5	±1.0	+1.8	−0.8	Underexposed	Fair
	1904									
136F	June 7.845	6.196	−31.2	+ 6.6	−24.6	±0.8	−4.6	+0.5	Good	Fair
137A	7.877	6.228	−30.3	+ 6.5	−23.8	±1.0	−2.9	+2.3	Fair	Good

that the actual observed velocities followed a superimposed curve with a period of 3.8 days and with nearly equal amplitudes for crests and troughs. For the better determination of the secondary curve the residuals of all plates from the ellipse have been plotted in Fig. 2 after being reduced to one complete period of 3.8 days, employing the well-established nodal point at 1.7 days after the light-maximum. A sine curve was then passed through these points with an amplitude of 4.2 km at the crest and 5.5 km at the trough, as shown in the diagram (Fig. 2). This final curve was superimposed on the velocity-curve and is represented by the heavy line of the upper curve of Fig. 1. The residuals for all the plates from this curve appear in Table VI under the heading *r*. Including all these residuals and assigning equal weights to each, the resulting probable error of a single plate is ±0.90 km. It can be seen by consulting the temperature-range in

Table V and the last two columns of Table VI that all the plates showing large residuals are undoubtedly inferior to the average plate. The long exposure and great temperature variation for 82A and the

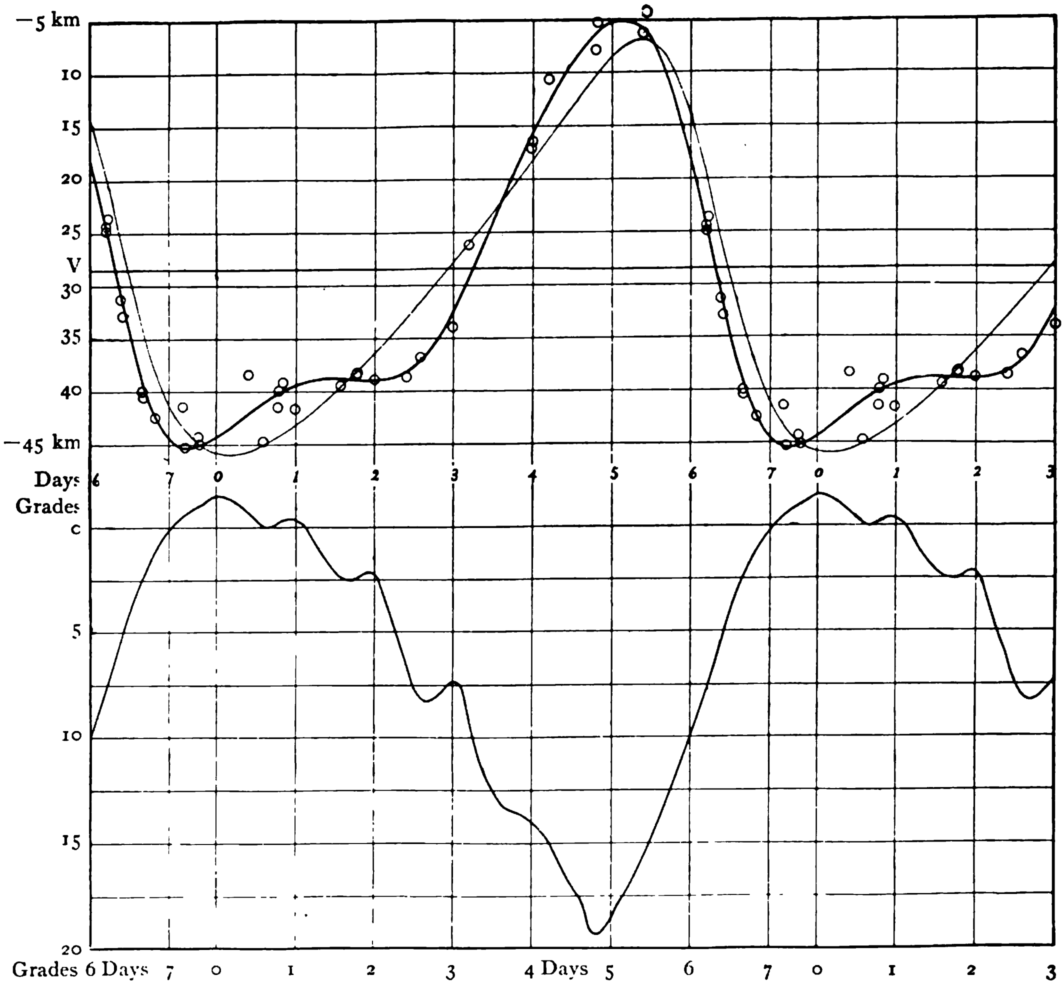


FIG. 1.—Velocity-Curves and Light-Curve of *W Sagittarii.*

poor comparison spectrum of 95 B would justify the rejection of these plates. Plate 91 A shows good spectra, but it should be noticed that its temperature-range is twice that of the average range for all the plates. Excluding five inferior plates whose velocities could be given

very small weight in the construction of the curve, the probable error of any one of the remaining twenty-eight plates is ± 0.55 km. The occurrence of relatively larger residuals along the crest of the secondary curve finds ready explanation when it is noticed that, of the two crests which obtain in one complete revolution of the system, the one occurs at the light-maximum where the greater activity of the star could lead to wide ranges in velocity, while the other occurs at light-minimum where, with increased exposure time, the effect of temperature-change in the instrument becomes maximum.

The elements of the orbital or primary curve of the brighter component of *W Sagittarii*, together with those of the superimposed secondary curve, are as follows:

TABLE VIII.

Elements.

		Primary Curve	Secondary Curve
Apparent period	P'	7.59460 days	3.80 days
Longitude of periastron	ω	70°0	
Eccentricity	e	0.320	0.0
Time of periastron after light maximum	T	6.20 days	
Projection of semi-major axis on plane of sight	$a \sin i$	1,930,000 km	
Projection of periastron distance on plane of sight	$q \sin i$	1,310,000 km	
Projection of apastron distance on plane of sight	$q' \sin i$	2,550,000 km	
Ratio of masses	$\frac{m^3 \sin^3 i}{(m+m_1)^2}$	0.004990	
Amplitude of velocity-curve at crest	A	+21.6 km	+4.2 km
Amplitude of velocity-curve at trough	B	−17.4 km	−5.5 km
Velocity of center of mass of the system	V	−28.6 km	

On the basis of these elements, I have constructed on Fig. 3 the elliptical orbit of the brighter component of *W Sagittarii*, indicating the line of nodes by a horizontal line and the line to the Sun by a vertical line drawn downward. Further, at the various points where they occur I have indicated the light-maximum and light-minimum and the cross-points of the primary and secondary velocity curves.

Our present knowledge of the elements of *W Sagittarii* makes possible the introduction of two small corrections to the period of this star depending upon the velocity of light. The first is due to the

change in the distance between the Earth and the bright component of the binary system occasioned by their orbital motions, and may be regarded as the combined effect of the equations of light of the

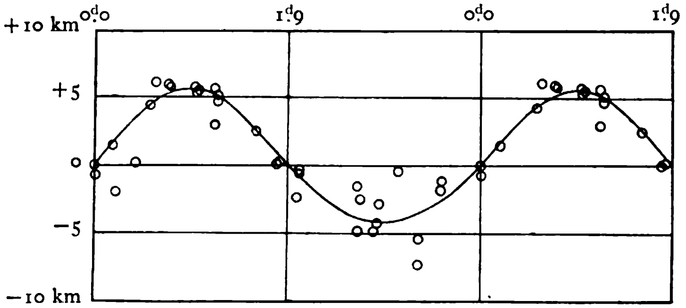


FIG. 2.—The Secondary Curve.

two bodies. Its operation is not systematic, but may be taken into account in the computed times for maxima used in plotting the velocity

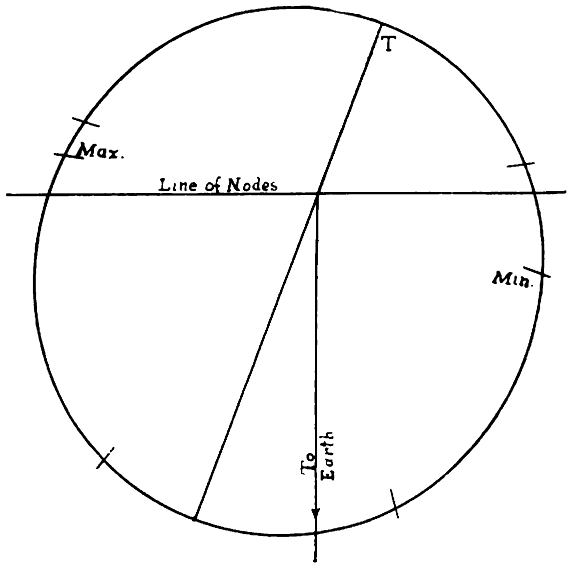


FIG. 3.—Orbit of *W Sagittarii.*

observations by changes never exceeding 0.01 days. The maximum variation from the mean in the present problem is about 0.002 days, which is equivalent to a displacement of 0.007 cm in the abscissæ of

my original velocity diagram. It is therefore evident that this correction is negligible. The second correction to the period of this variable is based upon the continual approach of the system toward the Sun at the rate of 28.6 km per second. As this correction is constant, the use of the apparent period for the velocity diagram does not affect the determination of the elements which characterize the form and position of the orbit. But the epochs of the curve referred to the time of maximum, and all dimensions of the orbit, are direct functions of the true period. The correction to the apparent period amounts to +62 seconds, or +0.00072 days. Accordingly, the true value of the light- and velocity-periods of *W Sagittarii* is

$$P=7.59532 \text{ days.}$$

As the remaining elements of the velocity-curve are published to three places only, this change in the fifth significant figure of P does not appreciably affect their values.

The angle i, which measures the inclination of the orbital plane to a plane tangent to the celestial sphere at the center of mass of the system, cannot be determined without micrometrical measures of the two components. The absolute scale of the ellipse is therefore indeterminate. For various assumptions of i the values of q are given below.

i	Periastron Distance
15°	5,060,000 km
30°	2,620,000
45°	1,850,000
60°	1,510,000
75°	1,360,000
90°	1,310,000

In the absence of any evidence regarding the radial velocity of the fainter star it is not possible to determine the masses of the system, but the constant of attraction furnishes data for the determination of these quantities when the angle i and the ratio of the masses are known. On the basis of various assumptions for i and m_1/m, I have tabulated below (Table IX) the corresponding values of the masses, in units of the Sun's mass, of the two stars (m and m_1) together with their distance apart at perihelion (q_1+q). $m_1$and q_1 refer to the bright star. The computations are based upon the equation

$$\frac{m^3 \sin^3 i}{(m+m_1)^2}=0.00499\odot .$$

TABLE IX.

m_1	i	15°	30°	45°	60°	75°	90°
m							
0.25	m	0.45 $\odot$	0.062	0.022	0.012	0.009	0.008
	m_1	0.11 $\odot$	0.016	0.005	0.003	0.002	0.003
	$q+q_1$	6,320,000 km	3,270,000	2,310,000	1,890,000	1,700,000	1,640,000
0.50	m	0.64 $\odot$	0.090	0.032	0.017	0.012	0.011
	m_1	0.16 $\odot$	0.045	0.016	0.009	0.006	0.006
	$q+q_1$	7,590,000 km	3,930,000	2,770,000	2,260,000	2,040,000	1,960,000
0.75	m	0.88 $\odot$	0.121	0.043	0.024	0.017	0.015
	m_1	0.66 $\odot$	0.091	0.032	0.018	0.013	0.011
	$q+q_1$	8,850,000 km	4,580,000	3,240,000	2,640,000	2,380,000	2,290,000
1.0	m	1.15 $\odot$	0.16	0.057	0.031	0.022	0.020
	m_1	1.15 $\odot$	0.16	0.057	0.031	0.022	0.020
	$q+q_1$	10,120,000 km	5,240,000	3,700,000	3,020,000	2,720,000	2,620,000
2.5	m	3.5 $\odot$	0.49	0.17	0.095	0.068	0.061
	m_1	8.8 $\odot$	1.23	0.43	0.237	0.170	0.153
	$q+q_1$	17,700,000 km	9,170,000	6,470,000	5,280,000	4,760,000	4,580,000
5.0	m	10.3 $\odot$	1.44	0.51	0.28	0.20	0.18
	m_1	51.7 $\odot$	7.20	2.54	1.38	0.99	0.90
	$q+q_1$	30,400,000 km	15,700,000	11,100,000	9,060,000	8,160,000	7,860,000
7.5	m	20.08 $\odot$	2.9	1.3	0.56	0.40	0.36
	m_1	155.70 $\odot$	20.2	10.3	4.17	3.01	2.71
	$q+q_1$	43,000,000 km	22,300,000	15,700,000	12,800,000	11,600,000	11.100,000
10	m	34.7 $\odot$	4.8	1.7	0.03	0.67	0.60
	m_1	347. $\odot$	48.2	17.1	9.29	6.60	6.03
	$q+q_1$	55,700,000 km	28,800,000	20,300,000	16,600,000	15,000,000	14,400.000
15	m	73.6 $\odot$	10.2	3.6	2.0	1.4	1.3
	m_1	1104. $\odot$	153.	54.3	29.6	21.3	19.2
	$q+q_1$	81,000,000 km	41,900,000	29.600,000	24,200,000	21,800,000	21,000,000

In determining the most probable value of the true scale of the orbit from Table IX, there is opportunity for unlimited conjecture. However, in view of what is known and of what in addition will be accepted as most reasonable, the range of such speculation can be greatly diminished. In view of the fact that the spectrum of the fainter star is of vanishing order, the ratio of the light-intensities of the two stars would probably be greater than 1 to 3. Assuming, then, that the apparent surface brightness of the two stars is the same, we are led to conclude that the ratio of the masses is greater than, or equal to, 1 to 5, since this ratio varies as the three-halves power of the ratio of the apparent disks for equal densities for the two stars. We are thus quickly restricted to the last four rows in Table IX. Further the secondary curve of Plates VIII and IX possesses many features that point to its origin in the rotation of the brighter star. The varying velocity could be ascribable to a difference in brightness between two hemispheres of the star. Let us assume that the ratio

of brightness of the two hemispheres is roughly one to two and take the amplitude of the secondary curve as 5 km per second; then, on the basis of the rotation interpretation for the secondary curve of *W Sagittarii*, the radius of this body would appear to be about that of the Sun, if the inclination of the equatorial plane to the line of sight be about 90°. For other values of this inclination the corresponding values of the radius of this star, together with its mass in terms of the Sun's mass, appear in Table X, assuming equal density for the Sun and this solar-type star.

TABLE X.

i	R	m_1
15	2,700,000 km	57.5 $\odot$
30	1,400,000	8.0
45	1,000,000	2.8
60	800,000	1.5
75	700,000	1.1
90	700,000	1.0

Reasoning from conditions that exist in our own planetary system, it seems most consistent to assume that the inclination of the equatorial plane of the brighter star to the orbital plane is approximately 0°. Comparing Tables IX and X on this basis, it will be seen that the mass-values in the latter table for each inclination would be duplicated in Table IX with a value of m_1/m equal to five or six. Confining our attention to the sixth row of Table IX, we find the inclination still undetermined. However, as the maximum orbital velocity of the smaller mass assumes the large values of 500 km per second for an inclination of 15°, 250 km for 30°, and 160 km for 45°, we naturally turn to the higher angles. I therefore suggest the following probable limiting values of the quantities involved:

Inclination - - - - - - -	i	45°–90°
Mass of brighter star - - - - -	m_1	0.9 $\odot$ – 2.5 $\odot$
Mass of fainter star - - - - - -	m	0.2 $\odot$ – 0.5 $\odot$
Periastron distance of brighter star - -	q_1	1,300,000 km – 1,800,000 km
Periastron distance of fainter star - -	q	7,000,000 km – 9,000,000 km
Radius of brighter star - - - - -	R_1	700,000 km – 1,000,000 km
Radius of fainter star	R	500,000 km – 600,000 km

As the existence of a strong variation in a star's light may be considered an exception to the general rule, it is possible that the internal conditions of this system are correspondingly extraordinary and the assumptions above are far from the truth. However, they will illustrate the outcome of one continuous train of reasoning.

The failure of double-star observers to detect any duplicity in this star leads to the conclusion that the angular distance between the components is not greater than $0\overset{''}{.}15$. On the assumption that the greatest apparent distance between the two is 10,000,000 km, the inference may be drawn that the distance of the system from the Earth is greater than three and one-half light-years.

For purposes of comparison among spectroscopic studies of *Cepheid* variables, there exist, in addition to the present investigation, the excellent results of Wright and Bélopolsky in connection with their researches regarding the orbits of *δ Cephei*[1] and of *η Aquilae*.[2] A strong resemblance between the elements of these stars is at once evident. All show a pronounced eccentricity. Also, the time of closest approach of the bodies of the system occurs about a day before maximum brightness. The most marked analogies exist between Wright's elements of *η Aquilae* and those of *W Sagittarii;* indeed, with the exception of a small difference in eccentricity, the uncertainty of inclination, and a difference of 0.5 days in the period, the two orbits are essentially identical even in regard to the positions of principal maximum and minimum. It is interesting to notice that Mr. Wright has obtained no evidence of a secondary curve for *η Aquilae*, but such a curve would not have been recognized if its amplitude did not exceed 2 km, and in some positions no great distortion of the ellipse would occur with an amplitude of five kilometers for the superimposed curve. Referring to Mr. Wright's article, it should be noticed that the velocity-curve reproduced there is the empirical curve which was drawn for the purpose of determining the elements. His actual ellipse passes slightly below observation 11, not above it. It will also be noticed that his observations tend above the published curve from 1 day to 2.8 days after maximum and fall below from 2.8 to 4.5

[1] ASTROPHYSICAL JOURNAL, **1**, 160, 1895.

[2] *Ibid.*, **6**, 393, 1897. *Ibid.*, **9**, 59, 1899.

days. It therefore seems probable that some curve oscillating about an ellipse would represent the observations better.

Mr. Wright's observations extend over four months, and my own over a period of less than a year. It is therefore probable that any influence connected with the long-period irregularities in the light-recurrence of these stars would escape detection. Further study of these stars should reveal such phenomena.

The observed uniform correspondence between the light-variations and the orbital conditions of these three stars suggests the mutual dependence of the two phenomena. Whatever may be the cause or causes of variation in the *Geminids*, they may not operate in the same manner in the *Cepheids*. It therefore seems advisable to treat each group separately in any attempt to construct a theory for the explanation of their light-changes. I shall therefore advance some data regarding the most probable conditions that obtain in the system of *W Sagittarii*.

Darwin's expression for tidal potential is:

$$V=\frac{3m}{2r^3}\rho^2\left(\cos^2 z-\tfrac{1}{3}\right),$$

where r is the distance between the masses, ρ is the radius of the disturbed body, and z the angle between r and ρ; m is the mass of the disturbing body. Confining our attention to the line joining the two bodies, we place $z=0°$ and differentiate with reference to the direction of displacement and arrive at the expression, $F=2m\rho/r^3$, where F is the tidal force. As the ratio of apastron to periastron distance in this system is 2 to 1, it is evident that the tidal force varies in the ratio of 1 to 8 for these two positions. Using the rough conclusions as to the most probable data for the system arrived at above, we find that the intensity of the tidal force acting on the brighter body is roughly 50,000 times that of the Moon acting on the Earth. Further, by the introduction of another term usually negligible in tidal computations, we find the ratio of the intensities of the tidal forces on the side toward the disturbing body to the corresponding forces on the opposite side of the brighter star is 15 to 10 at periastron and 13 to 10 at apastron.

While the increase of light may be due to enormous tidal disruptions in the molten matter of the star's surface, accompanied perhaps with a liberation of heat from below, it should be noted that the dis-

placements of the absorption lines in the spectrum are caused by the motions of the star's atmosphere with and relative to the regions emitting the light. In the atmospheric regions above the high tidal area there would undoubtedly be an uprush of gases due to atmospheric tides, convection currents, increased light-pressure, and explosive outburst. Again, in the belt of low tide ninety degrees from the two high tides, there would probably be a corresponding recession of the atmosphere toward the star's surface. This would clearly give rise, in the actual velocity-curve, to an oscillation with respect to a true ellipse. These oscillations would fall below the mean at conjunction and above the mean at elongation, for at conjunction the high tidal areas are presented to the observer and at elongation the low tidal areas. Consulting the diagrams, this is found to be the case. We can therefore depart from the rotation theory to account for the secondary curve; and indeed this seems justifiable, for it is possible, with the operation of such immense tides through long periods of time, that the rotation periods of the stars involved should be brought to identity with the revolution period, as in case of our own satellite. Further, on the basis of the tidal theory there would not seem to be sufficient variation in brightness due to difference in intensity of the tidal force between the two hemispheres (the ratio is about 7 to 5) to produce by rotation the oscillation observed, unless we assume much greater masses for the system. The occurrence of the light-maximum when the tidal forces have fallen one-third in intensity presents an anomaly, but it is possible that the effect of these forces lags to this extent.

Returning again to the rotation interpretation, it is easy to construct a plausible explanation for the light- and velocity-curves of *W Sagittarii* on the assumption that the system is pervaded by a resisting medium which enhances the brightness of that side of the star which faces the direction of motion. Or, again, a third body might be present in the system and give rise to the perturbations observed by Schmidt in the period. Until more data are available, it would be premature to follow out such theories.

Considering all evidence, it seems reasonably certain that the star's variations in brightness, and particularly the principal variations, are attributable to the action of external forces.

It is a pleasure to acknowledge my indebtedness to Director Campbell, who placed the necessary apparatus at my disposal, and gave continual counsel and encouragement during the prosecution of the work; and to Dr. H. D. Curtis and Dr. J. H. Moore for valuable advice and assistance.

LICK OBSERVATORY,
UNIVERSITY OF CALIFORNIA,
July 1, 1904.

SOME ADDITIONS TO THE ARC SPECTRA OF THE ALKALI METALS.

By F. A. Saunders.

Lenard[1] discovered a new series and several other lines in the arc spectrum of sodium. He removed the slit of a spectroscope and in its place focused a real image of the arc. With suitable dispersion he observed that different parts of the arc gave different "lines," and the new ones which he discovered were emitted by the hottest vapor, near the positive pole. Konen and Hagenbach[2] succeeded in photographing many of these lines and finding others in the lithium spectrum which are apparently emitted under similar circumstances. The writer, also, hoped to obtain photographs of such lines, using the usual slit, if his spectrograph were designed to give very bright spectra free from astigmatism, and if an image of the proper part of the arc were cast on the slit. The attempt was made with all the elements of the lithium family and the results, which were partially successful, are given below.

The essential feature of the apparatus used was a Rowland concave grating of about 10 cm width and 305 cm radius, ruled with lines of somewhat unusual length on a parabolic surface. The ruled surface had an area of 5 by 8 cm. The grating was mounted with the slit close to it on a solid iron casting bolted to a brick wall. An arm, supported from this casting and constructed of heavy gas-pipe, carried the camera at its end and could be turned about a point immediately below the grating. The table carrying the grating turned with this arm. The light from the slit fell on a parabolic mirror, similar to the one on which the grating was ruled, placed at such a distance that the reflected light formed a parallel beam. This then fell upon the grating, and the spectra were formed about 150 cm from the grating along a curve which was nearly a circle of 75 cm radius. The incident and reflected beams at the mirror made an angle of 3° with one another, while the angle between the axis of

[1] *Annalen der Physik*, **11**, 636, 1903.

[2] *Physikalische Zeitschrift*, **4**, 592, 801, 1903.

the grating and the beam incident on it varied from 12° to 21°, depending on which order was being photographed. The axis of the grating was adjusted to meet the photographic film at its middle point. By rotation of the arm, this relation remained unchanged, but the focal distance changed with the angle. The camera was accordingly made adjustable along the normal to the grating, and by careful trial three positions were obtained at which the camera could readily be set and clamped, so as to obtain spectra in good focus whose middle points lay near λ 3900, 5900, or 7800. The whole mounting was enclosed by a wooden structure with doubly-curtained door, which the observer could enter or leave during an exposure without fogging the film.

With this arrangement the spectra were almost entirely free from astigmatism for a space of 25 cm, corresponding to nearly 2,700 tenth-meters in the first order. Economy of light was insured by the short focus of the mounting and the large size both of grating and mirror.

The spectra were photographed on films 35 cm long and usually 2.5 cm wide. These were mounted in a holder which bound the film all along both edges and forced it to take such a curvature that the spectra were in focus along its whole length. This holder could be moved vertically by measurable amounts by a couple of large screws, so that several spectra could be photographed above one another on the same film. Each film took in over 3,800 tenth-meters at once, with good definition over almost the whole length, if the adjustments were correctly made. The scale of the photographs was nearly 11 tenth-meters to the millimeter in the first order, and two sharp lines could be seen distinctly separated if they were 0.7 tenth-meters, or less, apart.

The films used were from the Seed Company and were coated with their 26 X emulsion. A few Eastman Non-Curling films were also tried and found very satisfactory for wave-lengths less than λ 5900. For photographing lines in the red, the Seed films were stained with a simple cyanin bath. The writer is glad to have an opportunity of thanking Mr. R. James Wallace of the Yerkes Observatory for the formula, furnished by him, for this staining process, and recommends it to others interested, as a very simple and efficient way of sensitizing

plates as far into the red as λ 8000 tenth-meters. Mr. Wallace's formula is as follows:

Cyanin solution in alcohol (1 : 500)	5 cu.cm.
Alcohol	30 cu.cm.
Water	60 cu.cm.
Ammonia	10 drops.

The plate should be bathed in this for two minutes and washed for one. It may be used at once, even before drying. Such plates do not keep for many days as a rule, though the writer has used some that were kept under very favorable conditions for a couple of months and found them still fairly good.

The source of light for the present work was usually the carbon arc, using 10 to 15 amperes, direct current. The writer is indebted to the International Acheson Graphite Co., of Niagara Falls, N. Y., for some unusually pure graphite rods with which all these spectra were taken. This graphite by itself gave only half a dozen lines (mostly *Ca;* no iron) outside of the band spectrum of carbon (which showed the "tails" beautifully), but when it was saturated with a salt solution, several lines of titanium came out, evidently from the graphite. These were not unwelcome, as they were always sharp, and, as their wave-lengths are given in Rowland's table of solar lines, they made excellent standards of measurement. The differences in wave-length between these lines in the Sun and in the arc are too small to be worth considering in the present set of measurements. Eye observations showed that the alkali metal spectra were particularly well developed in the arc when the graphites were well saturated with salt solution and were separated by only 2 or 3 mm. With such a source the most successful photographs were taken; the well-known lines of the elements were then very much strengthened and broadened and the newer lines made their appearance. Fairly good photographs were, however, obtained with the arc longer, so that the "flames" were fully formed, if the light were taken from near the terminal. Exposures of three hours' duration were taken in the effort to pick up new lines in the deepest red. If the source of light could have been maintained in its most efficient condition during the whole time, the results might have been more complete, but the task of keeping the image of such lively flames as form these arcs constantly on the slit proved impossible.

A few photographs were also taken of the spark spectra of some of these elements, both with and without self-induction in the spark circuit, but no differences were detected in the relative intensities of any of the lines of the spectra of the spark or arc.

The photographs were usually taken with half the slit exposed directly to the light, and the other half covered with colored glass. In the resulting spectrum, lines in the first and second orders could easily be picked out where these overlap, as the ultra-violet lines were half the length of the others. In the deep-red photographs, half the slit was open for a short time and then covered with red glass, the other half being so covered throughout. In this way, images of unknown red lines would form part of the *same* spectrum with known second order lines; no shift could occur to alter their relative positions, as the colored glasses were supported independently and could be changed without affecting the position of the slit in the least. In all cases the beam of light from the condensing (quartz) lens, passing through the slit, filled the mirror and grating completely. The condensing lens was rigidly fixed throughout. The writer, for these reasons, feels certain that the photographs obtained can be relied upon to show the true positions of the lines.

The measurement of the films was accomplished by means of a Gaertner micrometer microscope with a run of 5 cm, graduated to read to 0.005 mm. Its screw was investigated and found to possess no error large enough to be worth considering. A magnifying power of about 15 diameters was used. In measuring an unknown line, in every case measurements were taken on several standard lines, lying on both sides of it, and its position was calculated from each of these; ten settings were made on each line. As a rule, the wave-length of any line, as given, is the average of several such sets of measurements taken from different photographs.

In the following tables of the complete arc spectra of the alkali elements, the writer has given in the first column the series to which the line belongs (*P* for principal, *I* for first subordinate, etc.). In the second column are placed various values for the wave-lengths and opposite each, in the third and fourth columns, the error as estimated by each observer and the observer's initial letter. The following are the observers quoted: L., Lehmann (*Annalen der*

Physik, **5**, 638, 1901); Ld., Lenard; K. and R., Kayser and Runge; K. and H., Konen and Hagenbach; H., Hagenbach (*Annalen der Physik*, **9**, 729, 1902); E. and H., Exner and Haschek (*Wellenlängen-Tabellen*, 1902); L. and D., Liveing and Dewar; B., Lecoq de Boisbaudran; and S. for the writer. The custom of stating errors seems to vary with different observers. The writer believes that the errors of measurement proper are usually small compared with errors due to wrong interpretation of the photographic image. In his own experience, several settings on a diffuse line may have agreed with one another to less than 0.1 tenth-meter, while a different observer has made equally concordant measurements leading to a result 0.2 or more away. Where so many lines are broadened or diffuse, as in these spectra, the importance of this class of error will be easily seen. The writer's own estimates of error are not based on variations in his microscope readings. If they were, they would be half or a third as large. He has tried to fix the error at such a value that the chances are extremely small that the measured wave-length will differ from the true one by more than the amount given. These estimates have been formed by the help of test-measurements taken on accurately known lines, following the same method as with unknown ones.

It is usual to give an estimate of the intensity of each line along with its wave-length. This has not been done in the following tables, as such estimates have usually, especially for the greater wave-lengths, depended more on the sensitiveness of the photographic plate for each vibration than on the real intensity in the source of light. The lines of a series, of course, decrease in intensity with decrease in wave-length; those of the principal series more rapidly than the others. The first subordinate series is stronger than the second, and the new series lines are the faintest of all. Quantitative measurements of the real intensities of spectrum lines are much to be desired, but the writer does not know of any that are applicable to these spectra.

The writer was not aware until after this work was done that Konen and Hagenbach had already found the lines at λ 6240 and λ 4148, which form a new series (with λ 4636) in the lithium spectrum. He gives his values for the wave-lengths of these and a few other lines

TABLE I.

Lithium.

Series	Wave-Length	Error	Observer	Series	Wave-length	Error	Observer
II	8127.34	0.27	L.	I	4132.44	0.2	K. and R.
	8127.0	0.3	S.	II	3985.94	0.2	"
P	6708.2	0.2	K. and R.	III	3924		K and H.
III	6240.8		K. and H.		3921.8		E. and H.
	6240.3	0.4	S.	I	3915.2	0.2	K and R.
I	6103.77	0.03	K. and R.	II	3838.3	3.0	"
II	4972.11	0.1	"	I	3794.9	5.0	"
III	4636.14		H.	I	3718.9	5.0	"
	4636.04		K. and H.	I	3670.6	5.0	"
	4636.3	0.4	S.	P	3232.77	0.03	"
I	4602.37	0.1	K. and R.	P	2741.39	0.03	"
	4603.04	0.01	H.	P	2562.60	0.03	"
	4602.00		H.	P	2475.13	0.1	"
	4603.2	0.2	S.	P	2425.55	0.1	"
	4601.6	0.2	S.	P	2394.54	0.2	"
II	4273.44	0.2	K. and R.	P	2373.9		L. and D.
III	4149.1		K. and H.	P	2359.4		"
	4148.2	1.0	S.				

in the hope that they may be of value, especially as some of them differ by considerable amounts from the values already given. The line at λ 4148, such as it is, is visible in Fig. 6, Plate I of Kayser's *Handbuch*, Vol. II, immediately to the right of λ 4132.44 (which by a misprint is numbered 4273 in the figure).

The "line" at λ 4602 deserves especial mention. Kayser (*Handbuch*, Vol. II, p. 366; also Fig. 5, Plate II) regards this as a line heavily reversed and much broadened towards the red. Hagenbach regards it as a pair of lines, the weaker one being constantly reversed, the heavier occasionally; the separation of the two amounts to over a tenth-meter. He is, apparently, ready to believe that all the lines in this spectrum are pairs with so great a separation. It is easy to obtain photographs of the strong line at λ 6708, and others, in which the lines are sharp enough to show a doubling, if the components were even closer than a tenth-meter, and no such doubling has been observed. The curious reversals obtained by Hagenbach certainly call for an explanation, but it is difficult to adopt the one given by him in view of the absence of doubling in those lines where it would be most easily detected. The writer took a set of six photographs of the 4602 group on a film, using the second order spectrum (5.5

tenth-meters to the mm), and varying the exposures and amounts of vapor in the arc so as to furnish wide differences among the set. In these photographs, the points of maximum density in the image on either side of the "reversal" remain in constant positions, even though the amount of vapor in the arc is small, in which case they are separated from each other by an absolutely clear space on the negative. The conclusion seems unavoidable from these images that we have here to deal with no reversal at all, but with two lines: a strong one at λ 4603.2, much broadened toward the red, and a weaker one at λ 4601.6, broadened toward the violet, neither of them being ordinarily reversed. If this view be adopted, the spectrum of lithium shows another analogy to that of sodium, for, in the latter spectrum, immediately beside the pair in the first subordinate series which is homologous to *Li* 4602 lies a faint pair broadened toward the violet in a similar manner.

The 4602 group presents the same aspect in the spark spectrum as in the arc.

In the spectrum of sodium, the new series of Lenard has been successfully photographed and measured; a new term in the red has been added and a faint haze at λ 4372 was detected on one photograph which is doubtless the sixth member of the series. The pair at λ 4472 was so diffuse that the lines could not be seen separately; the setting was made on the middle of it. The writer's measurements differ considerably from those of Lehmann and of Konen and Hagenbach on several lines; a repetition of the measurements led to the same values. Konen and Hagenbach give a line at λ 4973 which does not appear with certainty on any of the present photographs; nor does there seem to be any companion to λ 4660. The pair at λ 7410 is exceedingly faint, and may possibly not belong to sodium. It has, however, approximately the same separation as the Lenard series pair near by, and, like that pair, the line of greater wave-length is slightly the stronger. If this is a member of another series, it would seem likely that the other lines are too faint to have been observed, unless, possibly, it could be grouped with the pair at λ 5670 and the lines at 4975 and 4660. An extremely faint group was observed at λ 8210, but it was impossible to determine just what it was; this was the greatest wave-length which was photographed on these films.

TABLE II.

Sodium.

Series	Wave-Length	Error	Observer	Series	Wave-Length	Error	Observer
I	8194.76	0.2	L.		4820		Ld.
	8196.1	0.4	S.	*II*	4752.19	0.15	K. and R.
I	8184.33	0.2	L.	*II*	4748.36	0.15	"
	8184.5	0.4	S.		4730		Ld.
	7418.3	0.4	S.	*I*	4669.4	0.5	K. and R.
	7410.0	0.4	S.	*I*	4665.2	0.5	"
III	7377.4	0.4	S.		4660		K. and H.
III	7369.4	0.4	S.		4660.2	0.5	S.
II	6161.15	0.1	K. and R.	*III*	4633.1		K. and H.
II	6154.62	0.1	"		4629.5	1.0	S.
P	5896.16			*III*	4629.4		K. and H.
P	5890.19				4625.5	1.0	S.
I	5688.26	0.15	"	*II*	4546.03	0.2	K. and R.
I	5682.90	0.15	"	*II*	4542.75	0.2	"
	5675.92	0.15	"	*I*	4500.0	1.0	"
	5670.40	0.15	"	*I*	4494.3	1.0	"
III	5531.7		K. and H.	*III*	4470		Ld.
	5532.7	0.4	S.		4472.5	2.0	S.
III	5527.1		K. and H.	*II*	4423.7		L. and D.
	5528.2	0.4	S.	*II*	4420.2		"
II	5153.72	0.1	K. and R.	*I*	4393.7		"
II	5149.19	0.1	"	*I*	4390.7		"
	5100		Ld.	*III*	4372	5.0	S.
I	4983.53	0.2	K. and R.	*II*	4343.7		L. and D.
I	4979.30	0.2	"	*I*	4325.7		"
	4976.1		K. and H.	*P*	3303.07	0.03	K. and R.
	4975.0	0.4	S.	*P*	3302.47	0.03	"
	4973.0		K. and H.	*P*	2852.91	0.05	"
III	4913.5		"	*P*	2680.46	0.1	"
	4918.4	1.0	S.	*P*	2593.98	0.1	"
III	4910.1		K. and H.	*P*	2543.85	0.1	"
	4914.0	1.0	S.	*P*	2512.23	0.2	"

The lines λ 5100, 4820, and 4730 mentioned by Lenard could not be found on the photographs, nor were eye observations with a plane grating any more successful.

It may be worth noting that the wave-number differences for Lenard's series are as follows: 14.72, 14.77, 18.2, and 18.7. As the first two of these are probably accurate to one part in 80, while the last two are much less accurate, it seems fair to say that these differences are probably much smaller than the value for the usual series (17.2).

Many unsuccessful attempts have been made to find a simple formula which would express Lenard's series. The formulæ of Kayser and Runge, Rydberg, Fowler and Shaw[1], and Ritz[2] and modi-

[1] ASTROPHYSICAL JOURNAL, **18**, 21, 1903.

[2] *Annalen der Physik*, **12**, 264, 1903.

fications of these have been tried. Unless the formula contained four adjustable constants, it could not be made to fit the observations with any degree of precision. As almost any series, if not too accurately known, could be represented by a four-constant formula, no results of this work are given.

TABLE III.

Potassium.

Series	Wave-Length	Error	Observer
	7031.8	0.5	S.
	7699.3	5.0	K. and R.
P	7697		R.
	7701.92	0.52	L.
	7699.08	0.3	S.
	7665.6	5.0	K. and R.
P	7664		R.
	7668.54	0.52	L.
	7664.91	0.3	S.
I	6966.3	0.4	S.
	6938.8	0.5	K. and R.
II	6939		R.
	6939.5	0.4	S.
	6911.2	0.5	K. and R.
II	6913		R.
	6911.8	0.4	S.
I	5832.23	0.05	K. and R.
I	5812.54	0.05	"
II	5802.01	0.05	"
II	5782.67	0.05	"
I	5359.88	0.15	"
I	5343.35	0.15	"
II	5340.08	0.15	"
II	5323.55	0.15	"
I	5112.68	0.2	"
II	5099.64	0.2	"
I	5097.75	0.2	"
II	5084.49	0.2	"
I	4965.5	1.0	"
II	4956.8	1.0	"
I	4952.2	1.0	"
II	4943.1	1.0	"
	4870.8		L. and D.
I	4871.3	0.8	S.
	4870		R.

Series	Wave-Length	Error	Observer
	4863.8		L. and D.
II	4864.5	0.8	S.
	4862		R.
	4856.8		L. and D.
I	4856.8	0.8	S.
	4857		R.
II	4850.8		L. and D.
	4851.0	0.8	S.
	4829		R.
	4808.8		L. and D.
I	4803.8		"
	4803		R.
II	4801		R.
	4796.8		L. and D.
	4798		R.
	4788.8		L. and D.
	4767		R.
I	4759.8		L. and D.
	4760		R.
	4642.35	0.3	R.
	4642.5	0.3	S.
	4638.6		R.
P	4047.36	0.03	K. and R.
P	4044.29	0.03	"
P	3447.49	0.03	"
P	3446.49	0.03	"
P	3217.76	0.03	"
P	3217.27	0.03	"
P	3102.37	0.1	"
P	3102.15	0.1	"
P	3034.94	0.1	"
P	2992.33	0.15	"
P	2963.36	0.2	"
P	2942.8	1.0	"

In the spectrum of potassium, two new lines were found, one in almost the position predicted for it (see Ritz, *loc. cit.*). It is one of the hitherto missing first pair of the first subordinate series, which is, for some obscure reason, very faint. (Ritz has given reasons for believing this to be the first, rather than the second, subordinate

TABLE IV.

Rubidium.

Series	Wave-Length	Error	Observer	Series	Wave-Length	Error	Observer
	8513.26	0.26	L.	*I*	5259.8		B.
P	7950.46	0.32	L.		5260.51		R.
	7947.6	0.5	S.		5260.5	0.4	S.
P	7805.98	0.54	L.	*II*	5234.6		R.
	7799		R.		5234.0	0.7	S.
	7800.2	0.5	S.	*I*	5194.8		B.
Sat.	7759.5	0.5	S.		5195.76		R.
I	7753.58	0.54	L.		5195.9	0.5	S.
	7757.9	0.5	S.		5165.35		R.
I	7626.66	0.32	L.	*II*	5171	2.0	S.
	7619.2	0.3	S.		5161.8		B.
II	7406.19	0.25	L.	*I*	5151.20		R.
	7408.5	0.4	S.		5150.8	0.5	S.
II	7277.01	0.25	L.	*II*	5132		R.
	7280.3	0.3	S.		5133.5	0.8	S.
	6306.8		R.	*I*	5085.8		B.
I	6298.7	0.2	K. and R.		5089.5		R.
	6298.8	0.3	S.		5088.6	0.6	S.
I	6206.7	0.2	K. and R.	*I*	5076.3		R.
	6206.7	0.3	S.		5075.7	0.6	S.
II	6159.8	0.2	K. and R.		5037		R.
	6160.0	0.3	S.	*I*	5021.8		B.
II	6071.2	0.2	K. and R.		5023		R.
	6071.1	0.3	S.	*I*	5017		R.
I	5724.41	0.15	K. and R.	*I*	4983		R.
II	5654.22	0.15	"		4967		R.
I	5648.18	0.15	"	*P*	4215.72	0.03	K. and R.
II	5579.3		R.	*P*	4201.98	0.03	"
	5579.4	0.4	S.	*P*	3591.74	0.05	"
I	5431.83	0.15	K. and R.	*P*	3587.23	0.05	"
	5431.9	0.4	S.	*P*	3351.03	0.05	"
II	5391.3		R.	*P*	3348.86	0.05	"
	5391.2	0.4	S.	*P*	3229.26		R.
I	5362.94	0.2	K. and R.	*P*	3228.17		R.
	5363.1	0.4	S.	*P*	3158.7	0.3	S.
II	5322.83		R.				
	5323.1	0.5	S.				

series, as Kayser and Runge classified it.) The other member of this pair was not found, owing to the broadening of the line at λ 6939, near which it doubtless lies. The other new line (λ 7931.8) was so faint that its companion, if it is a member of a pair, could not be seen. The first term of the principal series was excellently photographed on several films and the writer feels considerable confidence in the value of the wave-lengths given for these lines. They are recorded to the second place of decimals, as the difference between the values

could be determined more accurately than the values themselves. The best measurements were taken on photographs where the potassium was present as an impurity and the lines were fine and sharp, though measurements on heavily reversed images of these lines gave concordant results.

The line at λ 4642 seems to be outside the series formation, and, along with the faintness of the first subordinate pair, offers a very odd peculiarity in this spectrum.

Three new lines were found in the spectrum of rubidium. One is a line at λ 3158 (not seen as a pair) belonging to the principal series; another, a very diffuse and faint line at λ 5171, which, with λ 5234, forms a pair in the second subordinate series. The third is a line at λ 7759.5 which is a companion to the first subordinate series line λ 7757.9. The results of the measurements in the deep red differ from those of Lehmann to a marked degree. As a check on the present methods, the writer took a set of measurements of several other and better-known lines in this spectrum. The results, which are included in the table, agree reasonably well with those of Kayser and Runge. As a further check, the wave-number differences for the pairs were calculated, using the writer's values for the wave-lengths. The results were:

P Series	Subordinate Series I	Subordinate Series II
237.8	234.7	237.7
	235.6	237.7
	235.8	237.1
	236.2	237.3
	236.3	232.8
	233.5	

The constancy of these numbers is satisfactory, with the exception of the short wave-length ends of the series, where the measurements do not pretend to be accurate. A similar table calculated from Lehmann's results shows a much wider divergence.

Ramage has noted a line at λ 6306; nothing was seen at this place except a ghost of the strong line at λ 6299. He has also given a sharp line at λ 5165; at this point on the writer's photographs appeared a sharp line which was the head of a carbon band.

TABLE V.

Caesium.

Series	Wave-Length	Error	Observer	Series	Wave-Length	Error	Observer
I	9211.86	0.7	L.	*Sat.*	6217.6		R.
IV?	9171.88	0.7	L.		6217.6	0.3	S.
P	8949.92	0.76	L.	*I*	6213.4	0.5	K. and R.
I	8766.10	0.32	L.		6213.1	0.3	S.
P	8527.72	0.32	L.	*II*	6034.43		R.
III	8080.02	0.48	L.		6034.8	0.3	S.
III	8019.62	0.48	L.	*I*	6010.59		R.
	8007.1	0.5	S.	*Sat.*	5847.86		R.
II	7944.7	0.3	S.	*I*	5845.31		R.
II	7616.58	0.44	L.	*II*	5839.33		R.
	7609.7	0.3	S.	*II*	5746.37		R.
IV	7280.5	1.0	S.	*I*	5664.14		R.
IV	7227.46	0.44	L.	*I*	5635.44		R.
	7228.8	1.0	S.	*II*	5574.4		R.
Sat.	6984		R.	*II*	5568.9		R.
	6983.8	0.3	S.	*I*	5503.1		R.
I	6973.9	5.0	K. and R.	*I*	5466.1		R.
	6973.1	0.3	S.	*I*	5414.4		R.
III	6869		R.	*II*	5407.5		R
	6872.6	1.0	S.	*I*	5351		R.
III	6829		R.	*I*	5341.15		R.
	6826.9	1.0	S.	*I*	5304		R.
I	6723.6	5.0	K. and R.	*I*	5256.96		R.
	6723.7	0.2	S.		5209		R.
IV	6630		R.	*I*	5199		R.
	6630.5	1.0	S.	*I*	5154		R.
IV	6588.0	1.0	S.	*P*	4593.34	0.05	K. and R.
II	6590		R.	*P*	4555.44	0.05	"
	6587.3	0.3	S.	*P*	3888.83	0.1	"
III	6472		R.	*P*	3876.73	0.1	"
	6475	2.0	S.	*P*	3617.08	0.3	"
III	6433		R.	*P*	3611.84	0.2	"
	6434	2.0	S.	*P*	3477.25		R.
IV	6359	3.0	S.	*P*	3398.40		R.
II	6354		R.	*P*	3348.72		R.
	6355.3	0.3	S.	*P*	3314		R.
IV	6325	3.0	S.	*P*	3287		R.

In the caesium spectrum, five new lines were found. The one at 7944.7 was predicted by Ritz and is a member of the second subordinate series, along with 7609.7.

Two new series can be arranged from the odd lines in this spectrum, which might be called the third and fourth subordinate series, as they evidently belong to this class.

Subordinate Series III	Subordinate Series IV
8080	?
8019	9171 ?
6872.6	7280.5
6826.9	7228.8
6475	6630.5
6434	6588.0
	6359
	6325

The wave-number differences of these pairs are approximately constant; they are: 96, 97.5, 97.7, 98.5, 98.4, and 80. The last pair, being on the verge of invisibility, is very inaccurately measured. These differences are much less than for the usual series, being about 0.177 as much. The line of greater wave-length in these pairs has slightly greater intensity. All the lines in these series are so exceedingly diffuse that their positions cannot be measured with much accuracy; it did not seem worth while on this account to attempt to fit a formula to them. From an inspection of their positions, it seems likely that they are not so directly connected with the first and second subordinate series as in the case of Lenard's series in the sodium spectrum. They appear to run together to a common end, which lies somewhat nearer the red than the end of the other series. It seems most unlikely from their appearance that they can be due to any impurity, as in that case they might fairly be expected to be sharp when faint; they are, however, always diffuse. These series are faint, and of approximately equal intensity.

It does not appear to have been previously noticed that the heavier line of each pair in the first subordinate series is accompanied by a satellite (lines all observed by Ramage) on the red side, forming a "secondary" series, using Rydberg's term. The writer has calculated the wave-number differences of the pairs in the caesium spectrum, using Lehmann's data for the two extreme pairs and his own for most of the rest. The results are as follows:

P Series	Subordinate Series I	Subordinate Series II
553.2	552.0	554.1
	531.9	554.2
	542.3	553.7
	547.2	554.6
	549.7	

It will be noticed that the values for the first subordinate series are lower than the others, and have an evident drift, with the exception of the first, which is possibly in error. As the first member of each pair is accompanied by a satellite (none have yet been observed for the first and last lines), the wave-number differences were calculated between the satellite and the line of shorter wave-length. The result is shown below:

Wave-Number Differences Calculated from Main Line	Wave-Number Differences Calculated from Satellite
531.9	553.7
542.3	553.9
547.2	554.7

It will be seen at once that the differences calculated from the satellites agree beautifully with those of the second subordinate series. Apparently, then, the wave-number difference for the first subordinate series, as usually calculated, should vary slightly, increasing with decreasing wave-length. A glance at the table of differences for rubidium shows the same effect in that spectrum also, and there, too, if we use the satellite at λ 7759.5 (the only one yet observed) with the line 7619.2, we obtain a difference of 237.3 instead of 234.7, thus bringing this value into agreement with those for the second subordinate series. This principle may be used to calculate the positions of the other satellites in the rubidium spectrum. They should be at λ 9213.6 (using Lehmann's line 8766.10 for the calculation), 6299.6 and 5725.1, thus being within very short distances of their parent lines. As the latter are broadened toward the greater wave-lengths they cover up the satellites, so that these have not yet been observed. The satellites in the spectra of lithium, sodium, and potassium are also, probably, too close to their parent lines to be distinguishable.

The writer wishes to express his indebtedness to the committee in charge of the Rumford Fund for a grant covering the expenses of this investigation.

Syracuse University,
June, 1904.

ON THE INVESTIGATION OF SIMULTANEOUS OCCURRENCES IN THE SOLAR ACTIVITY AND TERRESTRIAL MAGNETISM.

By A. NIPPOLDT.

AFTER a long period of purely statistical investigation of the connection between terrestrial magnetism and solar activity, an advance has been made in recent years by examining this relation in detail, *i. e.*, by investigating how a definite magnetic disturbance is associated with centers of solar activity simultaneously present. But in so doing it does not seem to be quite possible to entirely escape from the statistical method of the earlier date, and false conclusions have accordingly been reached. The present paper is intended to demonstrate this, with a special reference to the important studies of Father Cortie. It will be done by the discussion of a special case, to which Cortie assigns the greatest weight, and from which he reaches results entirely different from those of the writer.

In this JOURNAL[1] there recently appeared a paper by Father Cortie, entitled "On the Solar Prominences and Terrestrial Magnetism," in which he sought to prove from the magnetic records at Stonyhurst that a vigorously active Sun-spot had no effect on terrestrial magnetism. A paper in *Monthly Notices*[2] had a similar object; and an earlier investigation of the three years of minimum, 1899–1901,[3] is closely related with these. So far as known by the writer, Cortie's results have not yet been contradicted, and have already been cited in different places as undisputed, *e. g.*, by Deslandres.[4]

A powerful objection to any causal relationship between solar activity and terrestrial magnetism would be found in Cortie's attempted demonstration that there once occurred a Sun-spot of marked activity and lasted for two rotations of the Sun, which fell at

[1] ASTROPHYSICAL JOURNAL, **18**, 287–293, 1903.

[2] **62**, 516–521, 1902.

[3] ASTROPHYSICAL JOURNAL, **16**, 203–210, 1902.

[4] *Comptes Rendus*, **137**, 822, 1903.

a time of the greatest quiet and was accompanied by no magnetic disturbance. The following investigation is therefore devoted to this special case, the Sun-spot of May 19–June 26, 1901.

Cortie summarizes his result, so far as it concerns our question, in the following way: "that in the case of the only great Sun-spot of an otherwise absolutely quiet year, there is no connection between the solar storm and magnetic disturbances."[1] Three pages previously he established solely that "the magnets did not show even a moderate disturbance." As valuable as Cortie's service is, in that he gives greater importance to the investigation on the individual Sun-spot than to the statistical method, he nevertheless cannot quite emancipate himself from the latter, and he again introduces an idea so distinctly statistical as that of a "moderate disturbance." This idea had been previously developed by Ellis,[2] who, chiefly for statistical purposes, had to undertake some kind of a classification. Such a standard is, however, for the most part of value only for the purpose for which it was made, *i. e.*, here only for statistical ends. As is well known, Ellis chose the maximum amplitude as the basis for his classification of disturbances, and accordingly the disturbance of May 31, 1901, may actually not have been "a moderate disturbance." At Potsdam the maximum amplitude in declination was only $5'.4$, which is less than that of $10'$ fixed by Ellis as the lower limit of a moderate disturbance. The amplitude in horizontal intensity amounts, however, to 84×10^{-5} C.G.S. units, therefore exceeding by 34γ ($1\gamma = 1 \times 10^{-5}$C.G.S.) the lower limit for a moderate disturbance. We have 23γ as the maximum amplitude of vertical intensity. Therefore the disturbance at Potsdam must be reckoned at least as a moderate one. The schedule of Ellis fits only for places of similar geographical situation to Greenwich; at other places different upper and lower limits must be fixed.

The maximum amplitude can, indeed, hardly be usable for special investigations, in order to decide whether or not a curve is disturbed. One and the same magnetic disturbance may produce at one place a large, and at another a small maximum amplitude. For this we can only compare the registrations on the same day at different places.

In order to escape from the accidental character of numbers of this

[1] *Monthly Notices*, **62**, 521, 1902. [2] *Ibid.*, **60**, 142–157, 1899.

sort, another sort of classification—in five characters—was introduced by Eschenhagen, which has been adopted by many observatories in their regular publications. This classification is based on the whole general appearance presented by the curves registered: the more irregular it appears, the more disturbed is the course of the elements.[1]

Employing this method, the writer would say: *A disturbance occurs when the whole character of the variation becomes different from what it was before.*

Nothing illustrates this better than just this disturbance of May 31, 1901, now under discussion.

The variation in the horizontal intensity is shown in the accompanying figure, which was made from a contact photograph and was not in any way retouched.[2] The scale of time is omitted, because we are not concerned with it here. One mm in vertical distance corresponds to 9.73 γ. The original records in declination and vertical intensity are unfortunately too weak for reproduction.

The illustration begins at about midnight of May 30, and ends with the following midnight of May 31. The curves for all three elements have an entirely normal course until about the middle of the forenoon. Then there suddenly occurs at $9^h\ 13^m.5$ A. M., Potsdam Mean Time $= 8^h 20^m.7$ G.M.T., a typical outbreak of disturbance, and from this time on the whole character of the variation is entirely different from what it was before. There cannot be the slightest doubt that something has happened to the terrestrial magnetism which renders the normal course of the curve impossible. We must designate this condition as a disturbance, since it is of such an entirely different order from the preceding normal condition, even if the maximum amplitude in declination did not attain the value demanded by Ellis's scale. Now, since the

FIG. 1.—Curve of Magnetic Declination at Potsdam, May 30–31, 1901.

[1] (A more precise definition is given in the *Results of Magnetic Observations at Potsdam in 1890 and 1891.*)

[2] It has been necessary to reduce the illustration to one-third the size of the photographic print sent by the author.—EDS.

summary published by Cortie himself for the remaining days of the interval while the Sun-spot lasted indicated magnetic unrest frequently, we must unquestionably conclude that *at the time when that large Sun-spot was present, the terrestrial magnetism exhibited quite a number of disturbed days along with quiet days, and that therefore the conclusion cannot be drawn "that there was no connection between the solar storm and the magnetic disturbance."*

But even if no departure from the normal course of the terrestrial magnetism had been observed either at Stonyhurst or at Potsdam, or at any other observatory in middle and lower latitudes, the proof would nevertheless have to be supplied that no disturbances were observed also in high latitudes. *We could consider it as proved that a center of activity on the Sun was without influence on the terrestrial magnetism only when stations near the pole also exhibited no deviations from their normal curves.* Without committing ourselves to any one of the many new conceptions as to the nature of the effect of the solar activity upon terrestrial magnetism, we may be permitted to represent it as a sort of relay action: the strength of the releasing solar activity need not have a definite relation to the strength of the magnetic storm.[1] Thus, for instance, the large spot of October 12, 1903, was accompanied by only a relatively small magnetic perturbation, while the decidedly smaller spot of October 31 was associated with the largest disturbance ever observed. (At Potsdam the maximum amplitude and declination was $3^{\circ}\ 10'$, in horizontal intensity $950\,\gamma$, in vertical intensity $960\,\gamma$). The magnitude of the disturbance therefore cannot be decisive for our question.

Hence it was insisted, in what has preceded, that in all investigations of the relation between solar activity and magnetic variations there should not be any kind of statistical definition of the idea of disturbance, but the appearance of the curve should decide whether or not a disturbance occurred. A strong element of personality therefore enters into the matter. The force of this objection is somewhat diminished by the fact that each reader can form his own judgment from the illustrations accompanying any article or from the observed data at his disposal. If the case under consideration is a

[1] See also the attempted explanation by E. W. MAUNDER in *Monthly Notices*, **64**, 222–224, 1904.

sudden outbreak of disturbance, then at most only the time when the disturbance ended is uncertain. In other cases it might easily be possible for different investigators to be of different opinions. But this is also just the condition in regard to centers of solar activity. A gradual transition occurs between the large faculæ and the ordinary gaseous eruptions, and similarly there is a whole series of intermediate steps between the different courses of the magnetic variations. Indeed the automatic records of such sensitiveness as that attained by Edler at the Potsdam Observatory have revealed the fact that even an entirely normal curve is constituted of very numerous, though very small, oscillations; and that therefore here also, just as in the case of disturbances on a large scale, a force in violent variation discloses itself as the effective cause. This the author would associate as of the first importance with the uninterrupted activity of the Sun when free from spots.[1]

We therefore substitute for the statistical method, which can hardly furnish us with anything new, the investigation in detail; that that is, we make a study of each individual disturbance for itself in all its peculiarities. But it will not be possible to fully answer the questions as to the nature of the disturbances unless assistance is also offered by the astrophysicist to the magnetic observer. We must know what happened on the Sun at the time when definite phenomena occurred in the magnetic variations. As was recently shown by Deslandres,[2] the surveillance of the Sun as now maintained is inadequate for the purpose. The desired information can be supplied only by a continued and uninterrupted registration of the actions occurring on our central body.

MAGNETISCHES OBSERVATORIUM, POTSDAM.
July 7, 1904.

[1] Further particulars will presently be given in the *Meteorologische Zeitschrift.* See also the *Verhandlungen der Gesellschaft Deutscher Naturforscher und Aerzte zu Cassel.*

[2] *Comptes Rendus,* **137,** 821–827, 1903.

PRELIMINARY COMMUNICATION ON THE INFRA-RED ABSORPTION SPECTRA OF ORGANIC COMPOUNDS.

By William W. Coblentz.

The investigation of absorption spectra far into the infra-red has never been made in a thoroughly systematic manner. This is no doubt due to the enormous difficulties to be encountered and the slowness with which observational data can be obtained, so that usually after investigating half a dozen compounds, the results have been given to the public. As a consequence the agreement in the location of certain absorption bands is not very satisfactory, while some of the conclusions arrived at are not always convincing. This is partly due to the fact that the infra-red spectrum has never been examined farther than 7 μ for alcohols[1] and to about 10 μ for several other compounds. Now, it so happens, as will be shown later on, that with the limited dispersion at our disposal (if that be the true reason) all carbohydrates investigated show a large absorption band between the wave-lengths 3 μ and 3.5 μ, and then there are no marked bands till we arrive at about 7 μ. Beyond this, to the limit of the working transparency of rock-salt at 15 μ, there are numerous sharp, well-defined bands. Take then, for example, the question of the influence of the chemical structure of the molecule on absorption. Julius,[1] using a rock-salt prism, investigated about twenty compounds and found the absorption of isomeric alcohols quite similar from 3 μ to 7 μ.

Puccianti,[2] using a quartz prism to 2.5 μ, found the three isomeric xylenes so similar that a critical examination of his curves is necessary to convince one that structure influences absorption. The same is true of my own work on normal and iso-caproic acid, in which for the region from 3 to 6 μ the curves are identical. In general, it is only after one arrives at 8 to 12 μ that new bands occur. Of all the compounds studied, the only exception to the above statements is that of the mustard oils, *R-NCS*, and the sulphocyanates, *R-SCN*.

[1] *Verhandl. Konikl. Akad., Amsterdam*, Deel I, No. 1, 1892.

[2] *Nuovo Cimento*, 11, 241, 1900.

As will be pointed out later on, the mustard oils have an enormous absorption band at about 4.78 μ which occurs as a slight band at about 4.68 μ in the sulphocyanates, and is to be found in no other compounds, except as a moderately strong band at 4.6 μ in carbon disulphide.

This past year has been occupied, under an appointment as research assistant by the Carnegie Institution of Washington, in exploring the absorption spectra of at least one hundred and twenty compounds of carbon and hydrogen. Of this number about one hundred liquids or solids and fourteen gases were explored to 14 μ, using a rock-salt prism, while nineteen liquids were explored to 2.5 μ, using a quartz prism.

These data are as yet only incompletely worked up, and since there will be considerable delay in making the complete report, it has seemed wise to communicate this preliminary note. The problem before me was to determine the effect of molecular weight upon absorption; also the effect of chemical structure, *i. e.*, the arrangement of the atoms in the molecule, and the effect produced by the substitution of a CH_3 or OH group of atoms.

As a criterion for the effect of the substitution of a CH_3 group, the conspicuous band occurring between the wave-lengths 3.0 μ and 3.5 μ was critically examined. Julius found this band at 3.45 μ for compounds containing CH_3 groups, and hence ascribed it to this group. As a standard for judging the effect of the OH radical in certain compounds, the water bands found by Aschkinass[1] at 3 μ and 6 μ were selected. Ransohoff,[2] in his study of several alcohols, had tacitly concluded that the band at 3 μ was due to the OH radical. Such conclusions in regard to the CH_3 and OH groups seemed contradictory to the work of Ångström and Palmer,[3] who found that the Cl band at 4.28 μ does not occur in the six compounds investigated by Julius. The latter had previously shown that the chemical atom lost its identity in a compound, so that one cannot foretell the absorption spectrum of a compound from a knowledge of the spectra of the constituent elements. In addition to this we have the phenomena observed of solutions, in which the solute, sulphur[4] in carbon disulphide

[1] *Annalen der Physik*, **55**, 401, 1895.

[2] *Inaug. Dissertation*, Berlin, 1896.

[3] *Öfversigt Kongl. Vet. Akd.*, No. 6, 389, 1893.

[4] JULIUS, *loc. cit.*

and iodine[1] in carbon disulphide and chloroform, loses its absorbing power in the infra-red, and does not affect the selective absorption of the solute. In the optical region, however, the solution has a strong absorption band, which would indicate a resonance of small particles, as distinguished from the intra-molecular resonance of the solvent. The distinction between solvent and solute does not apply to mixtures of gases. The absorption spectrum of a mixture of CO and CO_2 is composed of the spectra of the separate gases. The same is true of the spectrum of illuminating gas, which is the composite of CO, CO_2, CH_4, C_2H_4, etc. From this it must not, however, be assumed that the gases are unique, for ethylene chloride shows bands belonging to carbon tetrachloride which it contains as an impurity, while toluene shows absorption bands common to thiophene, also present as an impurity.

The apparatus used in this work consisted of a 35 cm focal length mirror spectrometer, a 7 cm rock-salt prism, and a Nichols radiometer. Except for certain improvements, it is fully described elsewhere[2] and need not be mentioned here. A considerable portion of the work was repeated to 7.5 μ, using mirrors of 1 m focal length and 20 cm aperture, mounted on a large spectrometer.

With this large apparatus the spectrometer slits were 2′ of arc, while in the smaller they were 4′ of arc on the spectrometer circle, so that for the larger apparatus the dispersion was comparable to that of fluorite. With it numerous bands were resolved from 6 to 7 μ, but only occasionally were small bands found in the transparent region, already mentioned, from 4 to 5 μ, while the 3 to 3.5 μ region was sometimes found complex.

For the gases a glass cell 5.7 cm long was mounted in vertical ways, between the spectrometer slit and the incident energy, which was supplied by the heater of a Nernst lamp.

For the liquids different kinds of absorption cells were used. Those having boiling-points below 100° C. were placed in rock-salt cells, made by bending a fine wire, 0.08 to 0.16 mm in thickness, into a U-shape, covering it with Le Page's glue, and placing it between two plates of rock-salt. After drying, the glue was not attacked by the liquids examined. The top of the cell was covered with tinfoil. The plates of rock-salt were split from the natural crystal, about 2 × 3 cm on an edge, and were more satisfactory than those polished by hand.

[1] COBLENTZ, *Phys. Rev.*, **17**, 51, 1903. [2] *Ibid.*, **16**, 35, 1903.

Liquids boiling above 100° C. could be used in thinner films, which was an advantage on account of their opacity. For these a ring of tin-foil 0.01 mm in thickness was placed between the plates of rock-salt. Around the outside edge of the plates was placed a strip of pure tin, which was 0.1 mm thick, and hence easily bent to fit the cell, thus preventing evaporation. This form of cell is much better than that used in previous investigations, in that it can be thoroughly cleaned, while a new tin-foil ring was used for each new compound.

A block of wood having an opening cut in it over which the rock-salt cell was securely mounted, was placed in vertical ways before the spectrometer slit. The radiation from the Nernst heater passed through the opening in the block and through the rock-salt cell into the spectrometer slit. A clear plate of rock-salt was mounted directly below this cell. In this manner no radiation except that which passed through the cell or clear piece of rock-salt could enter the spectrometer.

The method of observation consisted in projecting successive portions of the spectrum upon the radiometer vane and noting its deflection when the absorption cell was before the collimator slit, and also the deflection when the clear piece of rock-salt was substituted. The ratio of the deflection through the cell to that through the plate of rock-salt gave the transmission through the liquid directly, and more accurately than by finding the absorption of the empty cell, and deducting it. This also meant the reduction of the work by almost one-half. After two months' use the difference in absorption of a plate of the absorption-cell and the "clear plate" was only 3.2 per cent. beyond 3 μ, which is of no significance, since we are not concerned with the question of total absorption.

One of the chief difficulties in this work is to obtain pure chemicals, and it is of the greatest importance to prevent contamination while investigating them. The compounds were imported directly from Kahlbaum, and were the purest obtainable.

In addition to the usual chemical methods of purifying the gases, fractional liquefaction and fractional distillation, in liquid air, was used. By this method they were obtained quite pure, especially ethylene, which showed a purity of 98.8 per cent. The details of all this work will appear in the complete report.

EFFECT OF STRUCTURE.

In order to learn what effect a group of atoms in a molecule has upon infra-red absorption spectra, the most logical procedure is to study isomeric compounds, for the purpose of determining fully that the phenomenon is intramolecular, and after that, to attempt to locate the particular group of atoms suspected of causing the disturbance. As already mentioned, in many cases the spectra of isomeric substances are very similar until we extend our observations far into the infra-red. The examples selected for this paper are representative of all the isomeric bodies studied. The total number studied is so large and varied, while the change in the spectra of pairs of isomers is so marked, that there can be no question that this is due to structure rather than to impurities. In the case of thymol and carvacrol, $C_{10}H_{14}O$, the effect of changing the OH group manifests itself to a marked degree at 5 and 6 μ, while from 9 to 14 μ the spectrum is entirely rearranged.

In aniline, $C_6H_5NH_2$, and its isomer, picoline, $C_5H_4N(CH_3)$, the effect of structure is still more marked. The benzene band at 3.25 μ, found in aniline, is entirely obliterated by the one at 3.35 μ in picoline, while in the spectrum of picoline only one band, at 10 μ, is in common with that of aniline.

In the sulphocyanates, *R-SCN*, and the mustard oils, *R-NCS*, the effect of structure is still more pronounced. As previously mentioned, the small band of the sulphocyanates at 4.68 μ is completely outclassed by the 4.78 μ band in the mustard oils. As the band occurring from 3 to 3.4 μ is a characteristic of carbohydrates, so is this band a characteristic of the mustard oils.

Of all compounds studied, the mustard oils are unique in having an enormous absorption band in the region of short wave-lengths, this side of 5 μ. In carbon disulphide the first strong band occurs at about 6.7 μ, in methyl iodide at 11.35 μ, and in carbon tetrachloride at 13 μ.

In allyl mustard oil, C_3H_5NCS, using the large spectrometer, this band was found to be complex, being opaque from 4.5 μ to 4.9 μ with the maximum located at about 4.8 μ. Phenyl mustard oil, C_6H_5NCS, is still more interesting, since it contains the 3.25 μ band as well as several others belonging to benzene, and has in addition

this strong band of the mustard oils, located at 4.8 μ, just as though the CH and the CS ions were vibrating side by side, but independently of each other.

Other isomers like pinine and limonene, $C_{10}H_{14}$, have a great similarity until we arrive at 10 μ, while the two caproic acids are identical to 6 μ, and begin to show dissimilarity at 8 μ. Probably the most evident example of the influence of structure is in the aliphatic or chain-linked group of atoms, like octane, and the carbocyclic or ring compounds, like benzene. If we consider simply the ***number*** of atoms in the molecule, then benzene, C_6H_6, can be designated by the formula C_nH_{2-6n}, and can be classed with the chain series, C_nH_{2n-2}, C_nH_{2n}, and C_nH_{2+2n}. Hence, reasoning from the fact that, in the three groups of chain compounds studied, *all* the conspicuous bands occur in common, one would expect at least a few of these bands to occur in the benzene, C_nH_{2n-6}, series. But no such coincidence occurs (in Table I, benzene, octane, and tetracosane are shown as typical examples), and only after the substitution of CH_3 groups for H atoms in benzene do we find bands, *e. g.*, 3.43 μ, in common with those of the chain compounds. If, then, we had no knowledge of these compounds, gained from organic chemistry, the evidence presented in the benzene curve and in the curves of octane and tetracosane would be sufficient to show that we are dealing with two distinct classes of compounds.

EFFECT OF MOLECULAR WEIGHT.

In the visible spectrum, Schönn[1], using columns 1.6 to 3.7 m in length of methyl, ethyl, and amyl alcohol found a shifting of the absorption band of methyl alcohol at 0.6430 μ to 0.6515 μ in ethyl, and to 0.6591 μ in amyl alcohol. This was followed by Gerard Krüss,[2] who examined sixty-four different compounds dissolved in CS_2, $CHCl_3$ and C_2H_5OH. He found that by the substitution of a methyl, ethyl, oxymethyl, or carboxyl group in a compound the maximum of the absorption band is shifted toward the red. On the other hand, by substituting a nitro or amido group the absorption band is shifted toward the violet. Subsequent writers on the subject of absorption spectra, in quoting this work, always mention the shift

[1] *Wied. Ann.*, (2) **6**, 267, 1879. [2] *Zeit. f. Phys. Chem.*, **2**, 312, 1888.

toward the red, but rarely mention the shift to the violet. In quoting such a complete investigation which records two well-defined series of phenomena, apparently opposed to each other when considering the question of molecular weight, it seems highly desirable to have the complete observation rather than the part which fits the particular problem under investigation. This is especially desirable in work like that of Ransohoff[1], who thought that a small sharp band found at 4.9 μ in CH_3OH was shifted to 5.2 μ in C_2H_5OH, "which would be an example like that of Krüss." He found no shifting for larger bands.

The following is what Krüss observed for indigo:

TABLE I.

Shift to red	Indigo in $CHCl_3$	λ max. at 0.6048 μ
	Methyl indigo in $CHCl_3$	λ max. at 0.61917
	Ethyl indigo in $CHCl_3$	λ max. at 0.6526
Shift to violet	Indigo in $CHCl_3$	λ max. at 0.6048
	Nitro-indigo in $CHCl_3$	λ max. at 0.5858

	Water Sol.	Alcohol Sol.
Fluorescin[2]	λ max. 0.494 μ	λ max. 0.4808 μ
Dibrom	0.5048	0.5094
Tetrabrom	0.5159	0.5251

This is a shift of 0.0055 μ per atom of *Br*, which proportionality was found not to hold true.

In the present work the results agree with that of Krüss, in so far as it seems permissible to assume that the occurrence of a certain conspicuous absorption band in a different place is a real shift. Several of the benzene derivatives are the most noticeable examples (Table I). In benzene, C_6H_6, the maximum occurs at 3.25 μ and is shifted to 3.3 μ in toluene, $C_6H_5CH_3$, to 3.38 μ in the xylenes, $C_6H_4(CH_3)_2$, and to 3.4 μ in mesitylene, $C_6H_3(CH_3)_3$. In other words, by substituting three CH_3 groups for an H atom we have shifted the maximum from 3.25 μ to 3.4 μ. Of all the compounds studied, excepting the gases, this is the only example where such a supposed shifting occurs. For a shift toward the shorter wavelengths, the amido (amino) derivatives of benzene are the most conspicuous, just as found by Krüss. In aniline, $C_6H_5NH_2$, we find

[1] *Loc. cit.* [2] E. VOGEL, *Wied. Ann.*, **43**, 449, 1891.

the benzene band almost obliterated and the minimum shifted to 2.97μ just as in ammonia, while in picoline, $C_5H_4N(CH_3)$, we have the nitrogen band at 2.92 μ and a second band at 3.35 μ. It is to be noticed that in the xylenes $C_6H_4(CH_3)_2$ and in pyridine, C_5H_5N, the benzene band at 3.25 μ has not been entirely obliterated, just as though there were different resonating ions, benzene, NH_2, and CH_3, vibrating side by side. This is more evident in xylidine and the mustard oils.

In xylidine, $C_6H_3(CH_3)_2NH_2$, which has an NH_2, and two CH_3 groups, we have the representative bands found in ammonia, at 2.95 μ, and in compounds predominating in CH_3 groups at 3.43 μ. The structural formula of aniline indicates that in the original benzene ring an H atom has been replaced by an NH_2 group, while in picoline we have a double benzene ring, containing an N atom and a CH_3 group. The absorption spectra support this theory, for in the aniline spectrum we have the original benzene band at 3.25 μ and the NH_2 band found in ammonia, xylidine, etc., while in picoline we have the benzene band obliterated and the CH_3 band is substituted. The picoline band occurs at 3.35 μ, the mean of 3.25 μ and 3.43 μ instead of 3.43 μ. Can we say, then, that there is a *real* shifting of the 3.25 μ band of benzene in the xylenes?

It must be remembered that we are integrating through a complex band which with ordinary dispersion cannot be resolved with a bolometer or a radiometer. Hence, when we find the maximum of benzene shifted to 3.3 μ in anisol and to 3.4 μ in mesitylene, and find the original benzene band in aniline, benzaldehyde, etc., it is a difficult matter to decide whether we have a true shifting, or whether we have simply determined the center of gravity of the several unresolved bands. An excellent example of this type is thymol, which melts at 44°. The solid film gave a deep band at 3.2 μ. In the melted condition the film was more homogeneous, and two bands were found at, 2.92 μ and 3.43 μ respectively, instead of the mean at 3.2 μ. Other examples like this have been observed when the layer of liquid under examination was too thick.

Now, this occurrence of a new band beside the old one is just what Krüss observed for his solutions, except that the old band has disappeared and the new one makes it appear as though there was a

shifting of the maximum. It is well known that the eye is insensible to slight variations in intensity in the visible spectrum, and it may be that by his method of dilution the weaker band has disappeared.

In view of the fact that we have such a striking similarity between the phenomena recorded here and those observed by Krüss, it appears highly desirable to make a spectro-radiometric study of dilute solutions, say of indigo, and of methyl and nitro-indigo, in chloroform, to learn whether there is but one band or whether there are two, viz., the original one due to the indigo ion which disappears on dilution, and a second due to the methyl or nitro group of ions, just as in the present work on aniline we have the original benzene band, at 3.25 μ, and a second at 2.97 μ. As already mentioned, it has not yet been shown that the selective absorption of a solid in solution and the intramolecular absorption of the solvent are identical, but the question can be more fully settled by a study of the solutions, as just indicated.

There are other bands farther out in the infra-red which shift back and forth, just as noted above, but the original benzene bands are more numerous and not so well defined, so that it is difficult to discuss them. The most noticeable ones are those of the methyl sulphocyanate at 7.06 μ and 7.61 μ, which occur at 6.91 μ and 7.27 μ in ethylsulphocyanate.

However, in all the benzene derivatives studied, the occurrence of an apparently new band in the derivative does not always seem to be *new*, but simply that the derivative has brought about a condition within the molecule such that the original resonating ion has greater freedom.

In gases there is a more definite shifting of the absorption band lying between 3 and 3.5 μ, as shown in Table II:

TABLE II.

Gases	Maxima	Maxima
Acetylene, C_2H_2	3.08 μ	7.38 μ
Ethylene, C_2H_4	3.28	6.98
Ethane, C_2H_6	3.30	6.85
Butane, C_4H_{10}	3.42	6.85
Methyl ether, $(CH_3)_2O$	3.45	6.88
Ethyl ether, $(C_2H_5)_2O$	3.45	7.00
Methane, CH_4	3.31	7.70

In the region of 6.8 to 7 μ there is a somewhat similar shifting, but there is less regularity in the positions of the bands. Ångström[1] has shown that the occurrence of the CO_2 band at 4.28 μ and of the CO band at 4.59 μ invalidates the assumption that the position of an absorption band depends upon molecular weight.

Ransohoff's[2] work on the alcohols shows that for the alcohols there is no shifting with increase in molecular weight.

Within the limits of experimental error, Puccianti's[3] work for the region of 1.71 μ shows no shifting of the maximum of an absorption band.

In all my work on the different compounds like methyl, and ethyl iodide, nitrate, cyanide, aniline, etc., no shifting can be detected. To make this test conclusive for the marked band at 3.43 μ, this region was repeated for both compounds (*e. g.*, methyl and ethyl iodide), before setting the spectrometer for another part of the spectrum. In this manner a slight shift, noticed in methyl and ethyl iodide, which had been examined on different dates, several months intervening, was found not to exist, showing an instrumental error. This method of testing a series of compounds at one region of the spectrum on the same day is the only way to be certain of slight differences of wave-lengths.

Through the generosity of Professor C. F. Mabery, of the Case School of Applied Science, who presented me with twenty-five very pure distillates of petroleum, belonging to the series C_nH_{2n-2}, C_nH_{2n}, and C_nH_{2n+2}, a final test was applied to this perplexing question. The absorption spectra of two of these, octane, C_8H_{18}, boiling-point 118–120°, and tetracosane, $C_{24}H_{50}$, solid, boiling point 274–276° (50 mm), are given in Table I.

In these as well as in all the intermediate ones no shifting could be detected, although the greatest efforts were made to do so. This was not a little surprising, for according to the measurements on the alcohols by Schönn (*loc. cit.*), in the visible spectrum, a shift for this greater number of CH_2 groups should have occurred. Even if we assume that the shifting is least for the infra-red and increases as we approach the ultra-violet, unless the total shift for this increase of 16 CH_2 groups [Octane, $C_8H_{18}=CH_3(CH_2)_6CH_3$, tetracosane, $C_{24}H_{50}$

[1] *Öfversigt Kongl. Vet. Akad.*, No. 7, 331, 1890. [2] *Loc. cit.* [3] *Loc. cit.*

$=CH_3(CH_2)_{22}CH_3]$ is less than 0.03 μ, it is safe to assume that no shifting occurs. A shift of 0.03 μ at 3.5 μ is 20″ of arc on the spectrometer circle, and at 7 μ it is 1′ of arc, for the small spectrometer, while on the larger apparatus the values in divisions of the spectrometer circle are *twice as great*, viz., 40″ and 2′ of arc, so that it would have been impossible to escape detection, especially such sharp, well defined bands like the one at 3.43 μ and that at 6.86 μ.

An interesting fact to be noticed in this connection is that *all* the prominent lines found in the two oils just mentioned, are present in *all* the petroleum oils studied, as well as in many other compounds like myricyl alcohol, piperidine, etc. For a larger dispersion the transparent region at 4 to 6 μ remains so for some oils while in others numerous small bands were found.

The difference between the spectra of the oils (aliphatic series) and the benzene spectrum (carbocyclic series) has been noticed under the question of structure. The benzene spectrum as well as that of its methyl derivatives is banded, "channeled," *i. e.*, the lines occur in groups, just as Pauer[1] found for the ultra-violet. He found the bands of the benzene spectrum, which extend from 0.235 μ to 0.260 μ, condensed and shifted toward the visible spectrum for toluene, the xylenes, aniline, etc., and considers it due to increase in molecular weight. If we consider the center of gravity of the benzene bands at about 0.245 μ, and that of the methyl derivatives, *e. g.*, toluene at about 0.267 μ, this shift amounts to 0.02 μ, while for aniline it is about 0.05 μ.

THE EFFECT OF CERTAIN CHARACTERISTIC GROUPS OF ATOMS.

Having found that infra-red absorption spectra depend upon the internal structure of the molecule, and that their maxima are not influenced by molecular weight, the next step is to determine, if possible, what groups of atoms, or "ions," have the power of absorbing heat-waves. This is of considerable importance, since many recorded phenomena have been credited to the "resonance of the *OH* ion in the molecule." Aschkinass (*loc. cit.*) found the absorption bands of water at the wave-lengths 1.51 μ, 3.06 μ, and 6.1 μ. Although he says little about the sequence of the maxima, subsequent writers

[1] *Ann. der Phys.*, **61**, 363, 1897.

have laid great stress upon this harmonic relation, showing electromagnetic resonance. Marx[1], in measuring the dielectric constants of water for electrical waves, finds a double harmonic relation for the electrical region. Ransohoff (*loc. cit.*) found the alcohol bands harmonic at 1.71 μ, and 3.43 μ. Although the alcohols were "chemically pure," that is a different question from the one of having them "water-free," which he does not consider, and the bands found by him at 3 μ and 6 μ may be due to water. The higher alcohols like glycerin, even if they could be freed from water, are so hydroscopic that they are difficult to investigate.

In the present work alcohols were avoided because of their great opacity beyond 7 μ, as well as on account of the difficulty in freeing them from water. Only one alcohol was studied, viz., myricyl, $C_{30}H_{60}OH$, which is a solid obtained from beeswax. A very thin solid film was obtained by melting between two plates of rock-salt. The spectrum is very marked for several strong absorption bands which correspond to those in the petroleum distillates. The dispersion with the large spectrometer at 3 μ is comparable with that of fluorite, and hence comparison can be made with the work of Ransohoff, who found the maxima of the alcohols at 1.71 μ, 3 μ, 3.43 μ, and 6.06 μ. In the present work the maxima occur at 1.71 μ, 2.95 μ, 3.43 μ, and 5.8 μ. The water bands were found at 2.95 μ and 6 μ, so that the 2.95 μ band coincides with the one for water, while the one at 5.8 μ does not. The disagreement with Ransohoff at 3 μ may be due to inaccuracies in the dispersion curve of rock-salt, which passes through a double curvature at this point, and hence is difficult to determine.

Two explorations were made, the first with the myricyl alcohol just taken from the containing bottle. For the second examination several grams of the myricyl were heated to 110° for seven hours, in a drying oven, which was sufficient to expel any water present. Immediately after the heating, a thin solid film was examined, and the bands at 2.95 μ and 3.43 μ coincided exactly with those found previously, which would indicate that the 2.95 μ band is not due to

[1] MARX, "Potential und Dissociation in Flammengasen," *Ann. der Phys.*, **2**, 795, 1900 (also on Electromagnetic Resonance, *Wied. Ann.*, **66**, 600, 1898), after a lengthy discussion concludes that, although it is a plausible assumption, it has not been proved that electrolytic dissociation in a flame depends upon the effect of the electromagnetic resonance of the OH ion upon infra-red radiation.

water. Whether it is due to the *OH* group is a different question. If it is due to *OH*, then the 5.8 μ band should coincide with the water band found at 6 μ.

Numerous other compounds having *OH* groups were studied. In Table III are given a series of compounds which have absorption bands at about 3 μ.

TABLE III.

Compounds	Maxima	Remarks
Water, HOH	2.95 μ	Depth is 70 per cent. as found with larger spectrometer
Thymol.. / Carvacrol } $C_{10}H_{13}OH$	2.92	
Eugenol, $C_{10}H_{11}O\text{-}OH$	2.89	
Methyl salicylate, $CH_3OOC\text{-}C_6H_4OH$	3.1	Depth 70 per cent., probably the mean of the 3.25 and 2.95 μ bands.
Menthol, $C_{10}H_{19}OH$	3.0	50 per cent., comparison spectrum of H_2O at 2.95
Phenol, C_6H_5OH	2.97	60 per cent.
Ammonia, NH_3	2.92	30 per cent.
Pyridine, C_5H_5N	2.95	30 per cent.
Picoline, $C_5H_4N(CH_3)$	2.92	Band shallow, 3 per cent.
Piperidine, $C_5H_{11}N$	3.00	30 per cent.
Aniline, $C_6H_5NH_2$	2.97	70 per cent., very sharp
Xylidine, $C_6H_3(CH_3)_2NH_2$	2.95	50 per cent.
Pyrrol, $C_4H_4(NH)$	2.95	70 per cent.
Eucalyptol } $C_{10}H_{18}O$	2.90	30 per cent. oxide, does not contain an OH group
Terpineol } $C_{10}H_{18}O$	2.93	30 per cent.

In Table III it will be noticed that ammonia also has a band near that of water, and at a slightly less wave-length. Considerable time was spent in showing that it was not due to water-vapor. The gas was fractionally liquefied and distilled, and then placed in a glass pipette containing freshly heated calcium oxide, over mercury, for eight days. At the end of this time the absorption band coincided exactly with the one previously found, showing that the band is characteristic of ammonia. Furthermore, it will be noticed that the compounds containing the amido, NH_2, group, and certain ones containing nitrogen, have a characteristic band in this region. These compounds were dried with potassium carbonate which would have removed traces of water. Other compounds like the aldehydes and

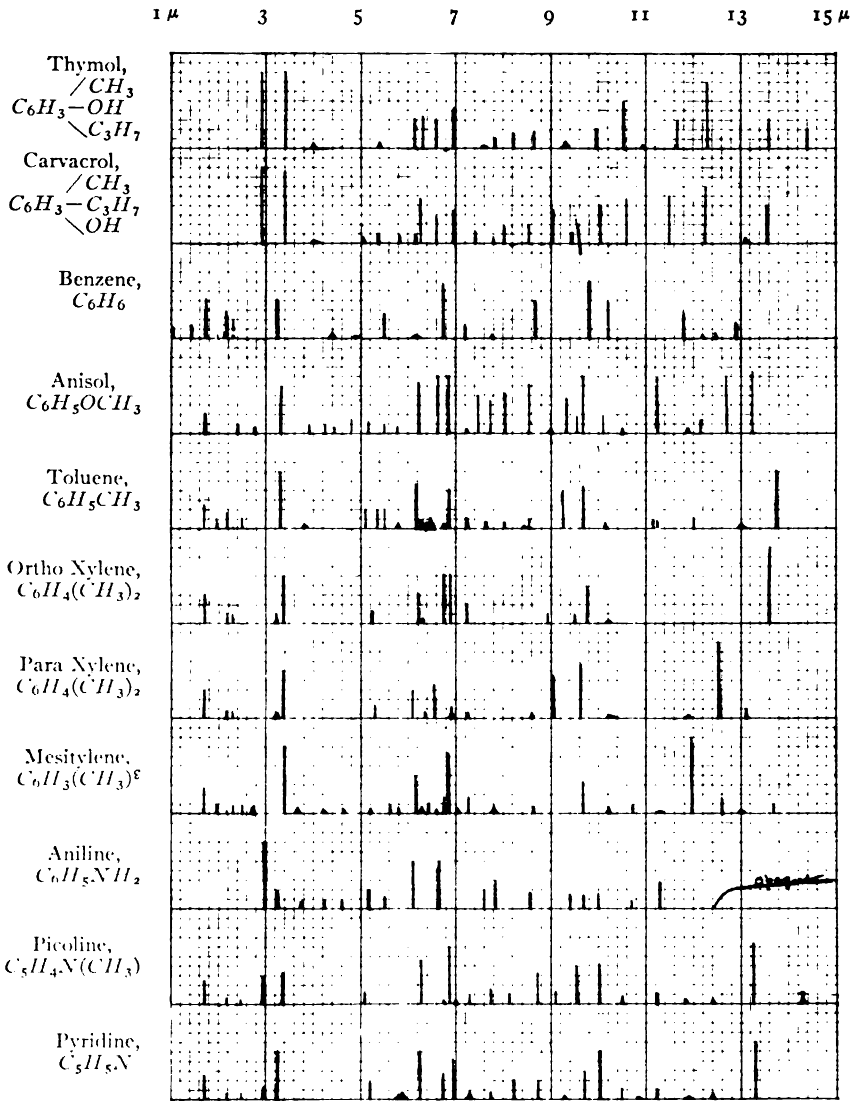


FIG. 1. —Absorption Bands in Infra-Red.

the fatty acids *do not* show this band. Commercial ethyl ether contains about 3 per cent. of water, but there is only a slight depression in the absorption curve at 2.95 μ. The fatty acids, *e. g.*, caproic, stearic, etc., are of interest because they have no band at 2.95 μ. In electrolysis, the alcohols are separated into ethyl and OH ions, while in the fatty acids, instead of the OH ion we have an H ion. Hence, reasoning from this analogy, one would not expect a band at 2.95 μ for the fatty acids. In the other compounds having an OH group, *e. g.*, eugenol, thymol, menthol, and phenol, strong bands are to be found, shifting from 2.89 μ to 3 μ. They show *no* bands at 6 μ. Can we assume, then, that the bands at 2.9 to 3 μ are due to OH? At the present writing the evidence is not very favorable. Considering the band of ammonia at 2.92 μ and those of compounds containing NH_2 or nitrogen, the coincidence appears to be somewhat accidental. Further in the infra-red we have numerous cases of the coincidence of absorption bands.

As a whole, the most definite conclusion we can draw at present is that the alcohols have a characteristic band at about 2.95 μ just as the band at 4.78 μ is characteristic of the mustard oils.

In myricyl alcohol the bands at 1.71 3.43, 6.86, 10.2, and 13.88 μ are closely harmonic. Puccianti (*loc. cit.*) observed that the band at 1.71 μ occurs in *all* compounds which have a C joined directly to an H atom. This has been verified in the present work, on seventeen new compounds, and it appears that this is a general case. But in benzene the next maximum occurs at 3.25 μ, and the following at 6.75 μ, which would indicate that the harmonic relation in other compounds is somewhat accidental. There are however, numerous pairs of lines, especially in ammonia, which from the "constant differences" of their vibration numbers indicate spectral series. A large band at 5.8 μ is of frequent occurrence in numerous benzene derivatives, as well as in the aliphatic compounds. The same is true of the 6.8 μ band found in all the petroleum oils, in a few benzene derivatives, and in piperidine, which consists of a ring of CH_2 groups. It thus appears that we have a certain vibrating ion which is present in numerous compounds.

The CH_3 group of atoms is probably the most important to be considered, but only a few cases can be mentioned here. The most

noticeable effect is in benzene derivatives. It was shown under the discussion of the effect of structure that the benzene group, C_6H_6, although it appears as a series, C_nH_{2n-6}, is entirely different from the chain compounds like C_nH_{2n-2}, etc. But a substitution of several

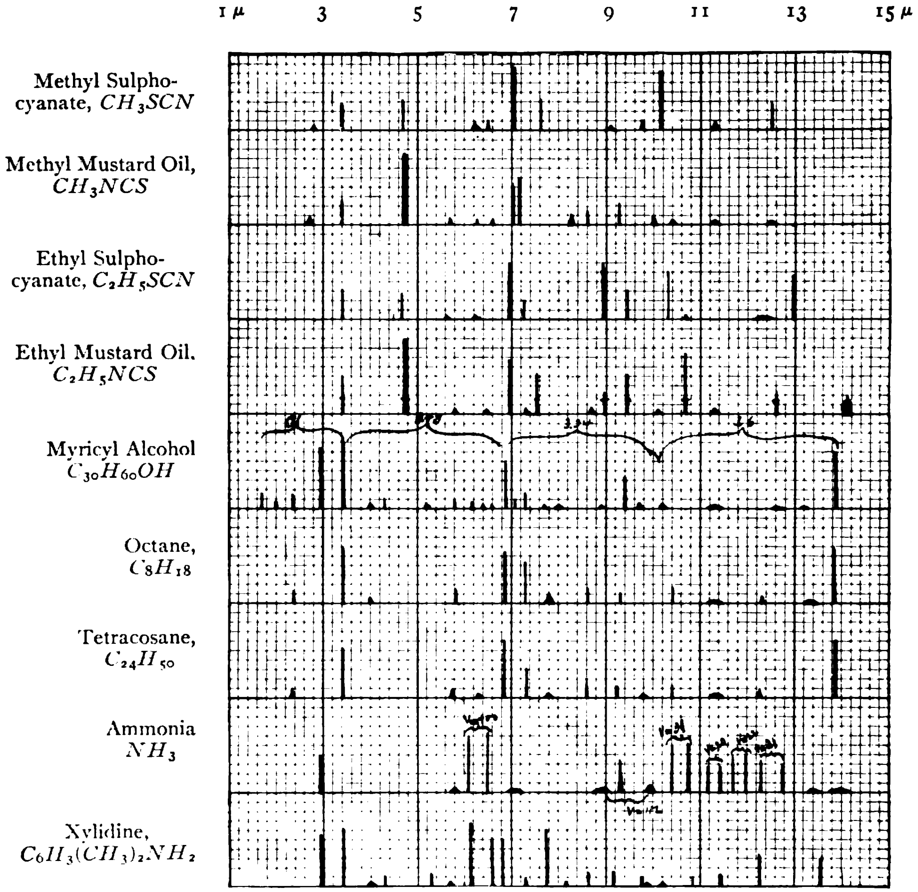


FIG. 2.—Absorption Bands in Infra-Red.

CH_3 groups completely absorbs the 3.25 μ (benzene) band, and the 3.43 μ band which is characteristic of all compounds containing CH_3 groups takes its place. Whether the 3.25 μ band has actually disappeared is an open question. In mesitylene there is still a trace of the 6.75 μ band of benzene, showing that the effect of the benzene

ion has not been destroyed by the substitution of three CH_3 groups. In the xylenes the 6.75 μ band is least affected, while the 3.25 μ suffers the most, and the whole strengthens the belief, mentioned in the beginning, that certain vibrating ions always seem to be present, but that their effect in absorbing heat-waves seems to depend upon the damping effect of surrounding groups of atoms. The effect of substituting an NH_2 group for an H atom, thus forming aniline, has the least effect on the benzene, 3.25 μ band, while those from 6 to 7 μ have entirely disappeared. In benzaldehyde, C_6H_5CHO, the 3.25 μ band is not seriously influenced by a more intense absorption maximum at 3.55 μ, while in benzonitrile, C_6H_5CN, and in C_6H_5Br the 3.25 μ band suffers no change.

Are we then to conclude that a certain group of atoms, which behaves in a certain definite manner in chemical reactions, absorbs heat-waves in the manner just noted; or are we to consider the effect due to the bonding of the atoms in the molecule; or is the effect due to both causes?

Only after a thorough study of the data at hand will it be possible to attempt a reply to this, as well as to numerous other questions not considered here.

Physical Laboratory,
Cornell University,
June 1, 1904.

Minor Contributions and Notes.

ON THE PHYSICAL NATURE OF THE SOLAR CORONA.[1]

In a paper on "The 1900 Solar Eclipse"[2] Langley and Abbot published some measurements on the heat radiation of the solar corona. This was found to be "unexpectedly feeble." Therefore the authors oppose the view that the "main source of the light from the corona is the incandescence of its particles, due to the proximity of the hot photosphere." Instead of this they suppose that the "principal source of its radiations is of the nature of an electrical discharge." The example used by the authors for such light caused by electrical discharge, namely, the light of the aurora, seems to me not to be a good one, for the aurora has a bright-line spectrum and the corona mainly a continuous one. Again, the phosphorescent glow under the influence of cathode or Röntgen—or ultra-violet—rays seems to give a nearer approximation to the continuous spectrum of the corona. But the spectra of the coronas of 1898 and 1901, as given by Campbell and Perrine, are not very similar to that of a phosphorescent substance, so that this modification of Langley and Abbot's views has also to struggle with rather great difficulties.

Mr. Campbell drew my attention to the conclusion of Langley and Abbot. As will be seen from the following considerations, the heat—and light—effects of the corona agree very well with the assumption that its radiation is due to the incandescence of its particles. The special difficulty of Langley and Abbot is given in the following words: "Why should the coronal radiation, of equal apparent brightness to that of the full Moon, be comparatively so feeble in heating effect?" This effect was measured by five scale-divisions on a bolometer, whereas a (black) body of the room's temperature under similar conditions gave eighteen divisions.

Now I shall show that the brightness and the heat-effect of the corona are just such as we might expect from dust-particles heated by the radiation from the photosphere of the Sun.

The image of the Sun was produced by a concave mirror of 1 m focal length. It had therefore a diameter of 9 mm. The part of the corona observed was 0.2 mm from the limb of the Sun. I assume that this means the edge of the photosphere. From this I calculate that the photosphere

[1] *Lick Observatory Bulletin* No. 58. [2] Washington, 1904, p. 26.

of the Sun, seen from the observed point of the corona, would fill an angle of sight equal to $2\overset{\circ}{.}73$, corresponding to a spherical angle equal to $2\pi \cdot 0.708$. A spherical particle in the corona at this point radiates to space through an angle of $1.292 \cdot 2\pi$, and to the Sun through an angle of $0.708 \cdot 2\pi$. Equilibrium of heat is soon reached and gives, in accordance with Stefan's law, if t is the absolute temperature of the particle, T that of the Sun (6000° absolute),

$$1.292\, t^4 + 0.708\,(t^4 - T^4) = 0;$$

from which $t^4 = 0.354\, T^4$, and as T is assumed to be 6000°, t is found to be 4620° absolute.

According to the law of Wien (or Planck) we may now calculate the relative strength of the radiations E_1 and E_2 of two bodies at the temperatures 6000° and 4620° for different wave-lengths (λ). Thus we find:

λ = 0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8
E_1 = 0.0041	2.0	142	248	270	245	202	162
E_2 = 0.0000033	0.052	13.2	41.7	64.9	74.7	73.0	66.1

Taking the sum of the radiations in the visible spectrum from $\lambda = 0.4$ to $\lambda = 0.8$, we find $\Sigma E_2 : \Sigma E_1 = 0.289$. If we only regard the physiologically most effective part in the spectrum, for $\lambda = 0.55$, we find $\Sigma E_2 : \Sigma E_1 = 0.271$, which falls very close to the first figure. We may use the mean of them, 0.28, as lying very near to the true figure.

A body of the temperature 4620° abs. would therefore radiate a quantity of light equal to 0.28 times that radiated from the Sun, if they took up equal spherical angles of sight. The same would be the case if the body of 4620° were composed of little particles in such numbers that the background (the sky) could not be seen between them. Now, instead of this, Langley and Abbot indicate that the brightness of the corona was equal only to that of the full Moon, which in accordance with the measurements of Zöllner, is supposed to be about 1:618000 of that of the Sun. Therefore we must conclude that only (1:618000):(0.28) = 1:173000 of the angle of sight to the corona is filled by the radiating particles of the corona, in the case of Langley and Abbot's observations.

In this calculation we have neglected the sunlight reflected from the particles in the corona. If we had not done so, we should have found a number somewhat, but not much, less than 1:173000.

Let us now make an analogous calculation for the radiation of heat from the corona. Langley and Abbot compare this with the radiation of heat from the Moon. Langley found[1] that the radiation of the Moon in the middle of the Earth's umbra is equal to 1.3, whereas the radiation from the

[1] *Memoirs Nat. Acad. of Sciences*, 4, Part II, p. 159.

middle of the full Moon is equal to 180 scale divisions. Now the middle of the full Moon has a temperature of about 419° abs. Calculating the temperature of the middle of the umbra according to Stefan's law, we find 122° abs. As the temperature of the Moon sinks from 419° abs. to 122° abs. in about three hours, it will be quite near to the truth to suppose that at the solar eclipse the center of the Moon will be very near to absolute zero, so that its radiation may be neglected. Now we will suppose that the temperature of the cardboard, used for screening the bolometer in the experiment, was +27° C.=300° abs., which will not be far from the truth. Further, we will suppose with Very[1] that the sky acts as a screen, sending back 50 per cent. of the radiation from the bolometer. Then the radiation from the bolometer to free space being —y*, it is to space with the intervening atmosphere of the Earth, $-\frac{1}{2}y$. This radiation gives a deviation of -18 divisions.

Now the bolometer was directed to the corona and the deviation found to be -13 divisions. The loss of heat to space through the atmosphere was as before $-y/2$. To this came the radiation from the corona, which is supposed to be absorbed by the atmosphere (or reflected by its dust-particles) to a degree of 50 per cent. Call it therefore $z/2$, so that we have

$$-y/2+z/2=-13$$

or $z=10$. Now we have $y=36$ as a measure of the radiation of a black body of 300° abs. If we suppose, as before, that the particles of the corona radiate as a black body, the effective temperature of the corona is found to be $\sqrt[4]{\frac{10}{36}}\cdot 300 = 217.75$ abs.

We have above deduced the temperature of the particles in the corona to be 4620° abs.; that is, the radiation should be $\left(\frac{4620}{217.75}\right)^4 = 2.02 \cdot 10^5$ stronger than it is found, provided that space would not be seen through the corona. Therefore we conclude that the particles of the corona do not fill more than the 202000th part of the angle of sight. This figure agrees excellently with that, 173000, calculated for the light, as evidently the measurements of Langley and Abbot are not claimed to give more than the order of magnitude. Also, the neglect of the sunlight reflected from the corona and the deviation of the small coronal particles from the laws of radiation valid for absolutely black bodies would well account for the discrepancy, if the measurements were absolutely correct.

[1] ASTROPHYSICAL JOURNAL, **8**, 265, 1898.

* The sign – indicates loss of heat from the bolometer.

The difficulty which Langley and Abbot found, as stated above, is discussed in the following quotation (*l. c.*, p. 251).

If the brightness of the full Moon and of the solar corona are to the eye the same, why should they be different in heating effect? Obviously a natural explanation would be the comparative absence of some invisible radiations in the case of the inner corona, which are present in the case of the Moon, such as would be due to the absence of infra-red rays.

We will analyze this point a little more closely. If we regard the radiation of the Sun as like that of a body with a temperature of 6000° abs., and count radiation between $0.4\,\mu$ and $0.8\,\mu$ as luminous, and the rest as non-luminous radiation, the proportion of luminous to the total radiation is as 0.45 : 1, calculated from Wien's law. If we now regard a point in the middle of the full Moon, we find that it reflects diffusely 12 per cent. of the Sun's light falling upon it (Zöllner), while the rest is transformed into heat. For the non-luminous radiation the Moon is supposed to be a nearly opaque body, and it reflects diffusely perhaps 2 per cent. of the incident non-luminous radiation and absorbs the rest. Now, all the absorbed radiation is (as equilibrium of heat is reached) radiated out as heat.° The proportion of the luminous and total radiation from the Moon is therefore in the proportion

$$\frac{0.12 \cdot 0.45}{1} = 0.054 : 1.$$

For the particles of the corona Wien's law gives the proportion of luminous to the total energy as 0.36 : 1, neglecting the sunlight reflected from the corona. We may therefore say that the luminous energy is more than 6.7 times stronger in the coronal light than in the moonlight, compared with the total energy. It is this distribution of the radiation which makes the corona appear so luminous and so "cold" compared with the Moon.

By help of the measurements of Abbot it is possible to form an idea of the mass of the coronal dust. The dust-particles in the inner corona are heated to 4620° abs., and may therefore be drops of liquid metal. It is not probable that the dust is of carbon, for carbon seems to have a rather high vapor-pressure at this temperature, and the gaseous pressure in the corona is extemely low. We may, for example, suppose that the dust-particles are molten iron drops (melting-point of iron about 1600°C.). The specific weight of iron is 7.9 at 0°C., of molten iron 6.9 (at 1600°C.), and if its cubical dilatation is like that of cast-iron (0.000053 between 0° and 1000°, according to Le Chatelier), its specific weight at 4620° abs. will be about 6

° Therefore it is for our calculation immaterial whether the non-luminous radiation is partially reflected or not.

(probably it is lower). If the iron drops reflected the incident light totally, they would be driven away by the radiation-pressure from the Sun with the same force as that with which they would be attracted to the Sun if their diameters were 250 $\mu\mu$. According to Schwarzschild's calculations for the influence of diffraction and the deficiency of the total reflection, this figure may bechanged to about 350 $\mu\mu$.

It is very probable that those drops for which gravitation is just compensated by the pressure of radiation will be the chief material of the inner corona. For drops of other sizes are selected out, the heavier ones by falling back to the Sun, the lighter ones by being driven away by the pressure of radiation, so that just the drops which, so to say, swim under the equal influence of gravitation and pressure of radiation will accumulate in the corona. The mass of such a drop is $m = \frac{4}{3}\pi(175)^3 \times 10^{-21} \times 6 = 0.135 \times 10^{-12}$grams. Their cross-section is $a = \pi(175)^2 \cdot 10^{-14}\,\text{cm}^2 = 9.62 \cdot 10^{-10}$ cm^2. Now we know that the particles in the corona fill about $\frac{1}{190000}$ of the angle of sight. Therefore a column of 1 m^2 cross-section through the corona at the point of observation of Abbot will contain $\frac{1}{190000} \cdot \frac{10^4}{9.62 \cdot 10^{-10}}$, $= 54.7 \cdot 10^6$ particles of a total mass equal to $7.37 \cdot 10^{-6}$ grams.

According to a formula by Turner, the brightness of the corona decreases in moving from the photosphere, inversely as the sixth power of the distance from the Sun's center. This may be due to a decrease of the temperature, but certainly the quantity of reflecting particles exerts the chief influence, and therefore I have supposed as a first approximation that the number of particles in a volume of 1 cubic meter is inversely proportional to the sixth power of the distance from the Sun. Then, according to Turner's formula, the concentration of matter in the innermost part of the corona would be 1.3 times greater than that calculated above. The total quantity of particles that we observe in the corona would, according to Turner's formula, be half as great as the quantity of particles in 1 m^2 at the innermost part of the corona, multiplied by the cross-section of the Sun in square meters. It would therefore be $\frac{1.3}{2} \cdot 54.7 \cdot 10^6 \cdot (108.56)^2 \cdot \frac{5.1}{4} \cdot 10^{14} = 62.8 \cdot 10^{24}$ particles with the total mass $8.6 \cdot 10^{12}$ grams.[1]

Now, the mass of the corona will be a little greater than this figure shows, for what we see is the total corona diminished by the part of it lying before and behind the Sun. I have executed a mechanical integration of the form-

[1] The surface of the earth is $5.1 \cdot 10^{14}$ m^2 and the radius of the Sun 108.56 times greater than the Earth's radius.

ula for this case, and found that the total mass of the corona is $\frac{667}{390}$ times greater than of that part calculated above, that is $14.7 \cdot 10^{12}$ grams. If we had supposed that another material than iron composes the corona, we should have found nearly the same total weight; so for instance, if the specific weight had been one, the total weight would have been about $11 \cdot 10^{12}$ grams. If the diffraction did not disturb, and the reflecting and absorbing power were always the same, then the calculated weights would be independent of the specific weight of the material of which the drops consist. For the total of the cross-sections of the drops, determined by the radiation, is an experimental constant, and the diameter of the drops would under these conditions be inversely proportional to their density. Now, the total mass is proportional to (total cross-section) $\times$ (diameter) $\times$ (specific weight) and as the first value is constant, and likewise the product of the last two, then the total product, *i. e.*, the calculated mass, is independent of the assumed specific weight. For different metals that may be supposed to enter into the corona the specific weight is also but little variable, and therefore also the condition for diffraction, according to Schwarzschild, is nearly the same for them all. Likewise their reflecting and absorbing power cannot be very different, so that the calculation given above may be regarded as very reliable concerning the order of magnitude.

As now the brightness of the corona seems to vary between the single and the double brightness of the full Moon, the total weight of it may vary in the same proportions, or approximately between 13 and $26 \cdot 10^{12}$ grams.

It is also easy to make a calculation of the probable number of drops in a cubic meter at the densest part of the corona. The number of drops in a cross-section of 1 square meter was found to be $54.7 \cdot 10^6$. The number of particles in a column of 1 square meter cross-section, and the Sun's diameter as height, would be 1.82 times greater (found by mechanical integration), if the number of particles per cubic meter were the same as in the innermost part of the corona. The total number of particles would be then $1.3 \cdot 54.7 \cdot 10^6 \cdot 1.82 = 130 \cdot 10^6$ in $108.56 \cdot 12740 \cdot 10^3 = 138 \cdot 10^7$ m^3, or every particle would take up a space of 10.7 cubic meters. This space would increase proportionately to the sixth power of the distance from the Sun's center, and in a distance of one solar radius would therefore be $64 \cdot 10.7 = 685$ cubic meters. If we had calculated with a substance having n times greater specific weight than the iron, we should have found a volume about n^2 times less, so for particles of the specific weight 12, the volume would have been only 2.7 cubic meters for every particle in the innermost part of the corona.

As a comparison with the corona we may use a dense fog. Conrad determined the quantity of water in a fog, in which one could see 26 steps (equal to about 20 m) to be 4.4 grams per cubic meter. For water drops of 20 μ diameter (probable value) 1 cubic meter of this contains $1050 \cdot 10^6$ drops, with a total cross-section of 0.33 m^2. According to this it is easy to calculate that a thin layer of 0.015 millimeter of this fog would look just as bright as the corona, if the drops consisted of molten iron. Through a wall of such a fog 20 meters thick only the 630th part of an incident light-ray would pass unreflected and unrefracted, which agrees well with the assertion that it is impossible to discriminate objects at 20 meters distance through such a fog.

It is often supposed that the outermost layers of the Sun are of an exceedingly low temperature, due to the adiabatic dilatation of the Sun's gases from their vertical circulation. Just in the same manner we may calculate that the highest strata of the Earth's atmosphere should have an exceedingly low temperature.

The spectroscopic evidence for the Sun gives a totally different idea of the temperature in its upper strata. This depends upon two circumstances. The radiation of the Sun is extraordinarily strong. In the higher strata the density and consequently the heat-capacity of the gases sink to the lowest limit. Therefore their expansion, with the lowering of the temperature in ascending, is wholly overwhelmed by the strong radiation, and we may calculate the temperature as determined by the radiation alone, as we have done above, without committing any sensible error.

This probably also holds good for the uppermost extremely thin strata of the Earth's atmosphere, especially on the insolated side of the Earth. These highest strata contain particles of cosmical dust, supposed to swim by help of the repulsion of their negative electric charges from the electric charge of lower strata. On account of the insolation the temperature of these dust-particles reaches about 57° C., if the temperature of the soil below is about 30° C.,[1] as is easily calculated by the formula of Stefan. Also, on the night side of the Earth, by the radiation of the Earth, these particles will get a temperature $\sqrt[4]{2}$ times lower than that of the soil. If this is assumed to be 15° C. one finds for the dust-particles in the highest strata −31° C. Now much lower temperatures have been observed in lower strata up to about 20 km. It is therefore probable that our atmosphere at a certain height reaches a minimum of temperature, and that at higher strata the temperature again increases. Especially is this valid for the insolated part of the Earth, on which the highest temperatures according to this

[1] Above a soil of 0° C. it would be 47° C.

opinion occur in the highest strata of the atmosphere and not, as is generally supposed, in the lowest layers of it.

These conclusions are in excellent agreement with the results of the most modern researches, by Teisserenc de Bort and Assmann, of the temperature of the highest investigated strata of the air.

SVANTE ARRHENIUS.

LICK OBSERVATORY, MOUNT HAMILTON,
August 1, 1904.

THE RADIAL VELOCITIES OF *S SAGITTAE* AND *Y SAGITTARII*.[1]

MEASURES of seven plates of *S Sagittae*, employing the low-power, sky-standard table published elsewhere in this number, have resulted in the detection of a wide range in the radial velocity of this star. As a whole, these plates are much below the average excellence, but are sufficiently reliable to establish the binary character of this variable. The accompanying table contains the number of the plate, the Greenwich date, the interval since maximum, the velocity referred to the Sun, the number of lines used for each plate, and the temperature-change during the exposure. If the velocities are plotted in the usual way, assuming the identity of the light- and velocity-periods, they are seen to follow a curve in every way similar to those of *η Aquilae* and *W Sagittarii*, and the elements will approximate closely to those determined for *W Sagittarii* above. There is also some evidence pointing to a composite character for the curve.

Plate No.	Date		Interval since Max.	V	Number of Lines	Temperature-Range	Remarks
28 F	1903 Aug.	9.9	$1^d.4$	$-20^{km}.3$	20	$0^{\circ}.4$C.	Underexposed
33 E		14.9	6.4	−32.2	44	0.1	
35 D		16.9	0.0	−30.2	45		
42 B		26.9	1.6	−20.2	20	0.3	Underexposed
54 B	Sept.	6.9	4.2	+3.9	41	0.1	Comparison poor on one side.
72 B		13.8	2.8	−17.3	37	0.0	
3350 C	1904 July	20	3.7	−3.9	9	0.0	Underexposed

As the result of relative measures of four lines on two plates, Dr. Stebbins found a range of 9 km in the radial velocity of *Y Sagittarii*. My own measures of nine plates of this star indicate that it is approaching our system, but not at a constant rate. The range of velocity so far observed amounts to 17 km. The form of the curve seems quite different from that of other

[1] Also to appear in a *Bulletin* of the Lick Observatory.

Cepheid variables, but the character and number of the plates available are not such as to warrant any definite conclusions on that point.

RALPH H. CURTISS.

LICK OBSERVATORY,
July 1904.

ON THE SPECTRA OF *R SCUTI* AND *W CYGNI*.[1]

OBSERVATIONS of the spectra of *R Scuti* and *W Cygni* are particularly valuable, as these stars occupy a unique position between the short-period variables on the one hand, and the *o Ceti* variables on the other. Visual observations of *R Scuti* by Espin have led him to suspect the presence of bright lines in its spectrum, but he seems to have been unable to identify them. As far as I know, *W Cygni* had not been observed with the spectroscope.

Exposures upon both these stars with Spectrograph I were begun in the middle of July 1903. They were continued until November 11, 1903, in case of *R Scuti*, and December 28, 1903, in case of *W Cygni*. *R Scuti* was examined visually with the spectrograph until December 7. In the meantime, *R Scuti* rose to maxima toward the end of July and the beginning of October, while maxima of *W Cygni* occurred early in August and in the middle of December. During this period about twenty-five spectrograms of *R Scuti* and twenty of *W Cygni* were secured with Spectrograph I.

On plate 13 E of *R Scuti* (see Plate XIV, 1), H_β, H_γ, and H_δ shone out strongly as bright lines, but they faded quickly until at minimum the spectrum of the star departed but little from the solar type with dark hydrogen lines of customary intensity. At the two subsequent maxima the bright hydrogen lines were not seen, but the intensity of the absorption line at H_γ seemed to have decreased very much as the star approached its maximum of October (see Plate XIV, 2). Judging from rough measures of a few plates, the radial velocity of this star appears to be constant and about +42 km in actual value.

W Cygni shows a banded spectrum of a type characteristic of long-period variables. At the maximum of August 1903 the hydrogen lines mentioned above appeared as strong bright lines (see Plate XIV, 3) which faded gradually to the star's minimum, while the absorption line at *g* broadened greatly during the same period. Other suspected bright lines in the spectrum of this star remain to be identified.

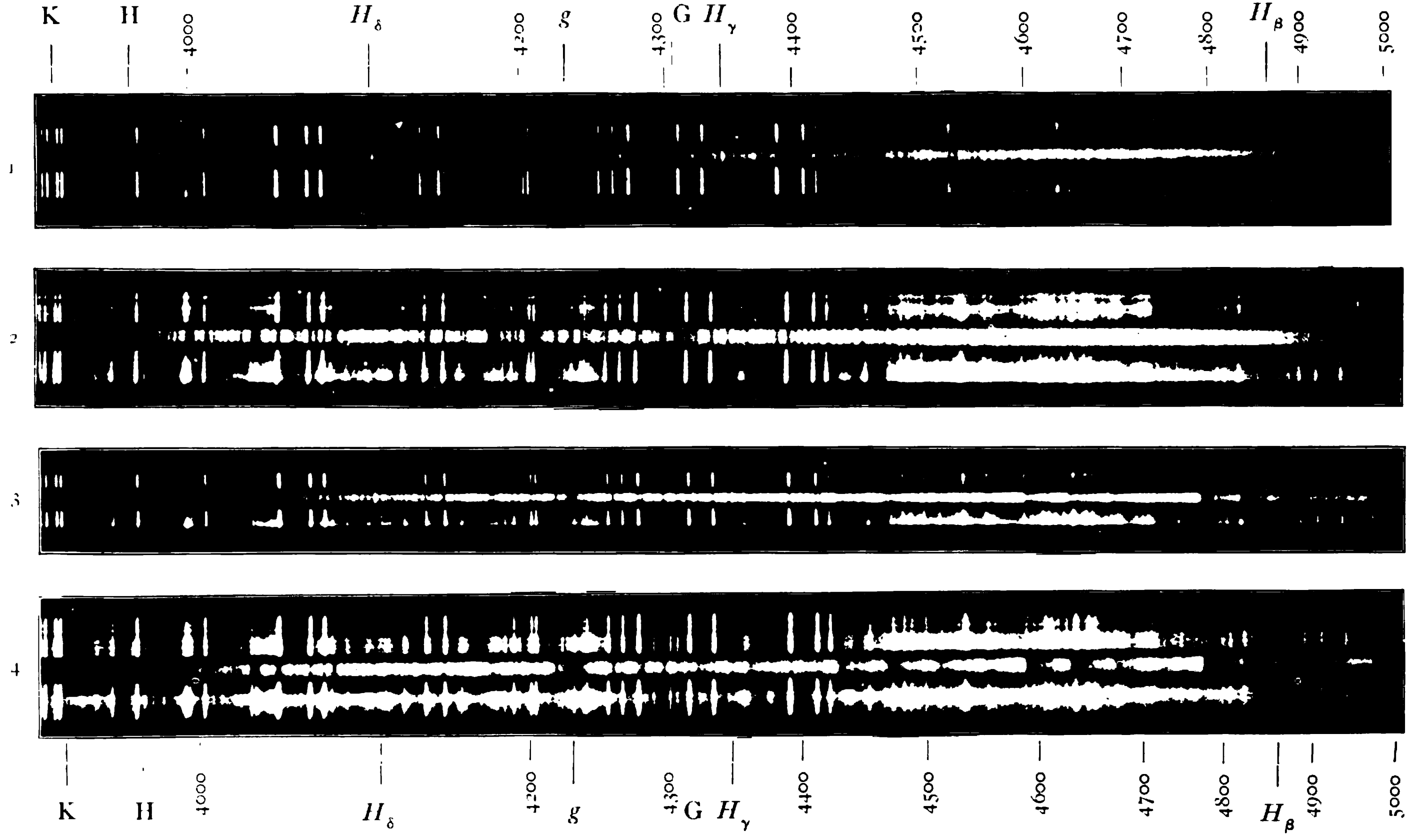
A detailed study of these plates will be made at the earliest opportunity.

RALPH H. CURTISS.

LICK OBSERVATORY,
July 1, 1904.

[1] Also to appear in a *Bulletin* of the Lick Observatory.

PLATE XIV.



1. *R Scuti*, 1903, July 19. 13 E.
2. *R Scuti*, 1903, September 21. 84 B.
3. *W Cygni*, 1903, August 3. 24 C.
4. *W Cygni*, 1903, October 12. 111 A.

SILICON LINES IN SPARK AND STELLAR SPECTRA.[1]

I have previously[2] shown that the spectral lines in the visible region of the oscillatory silicon spark may be divided into two groups, one of which includes those lines which are either unaffected or slightly enhanced by the introduction of inductance, while the other group includes those which disappear when inductance is used. These persistent lines of the first group, which I shall designate by P, have their origin in the "aureole" of the spark, and are doubtless due to a high temperature; those of the second group, which I shall designate by D, originate in the initial linear discharge, where it is probable that both dissociation and high temperature are at work.

By means of photography I have extended this research to the ultra-violet region and have compared these two groups with the corresponding lines found in stellar spectra by Lockyer[3] and Lunt.[4] The enhanced lines of silicon have been employed by Lockyer for making a tentative classification of stars on the basis of temperature; and my thought is that the effect of inductance upon these lines might shed some light upon this classification.

The spectra described below were obtained partly by use of two heavy flint prisms and partly by use of a single Rutherford prism. For purposes of comparison I have employed the ordinary spark spectrum of silicon, the spark-gap being shunted with a capacity of 0.009 microfarads. Alongside this spectrum was photographed that of the same spark after successive inductances, varying from 0.00002 to 0.03 henry, had been introduced. Wave-lengths were determined by comparison with those of a lead-cadmium alloy photographed on the same plate. The silicon employed was partly some which had been crystallized in small octohedra and plates, and partly sodium silicate cast in tubes about a platinum wire as core.

In the following table, the Roman numerals indicate the temperature groups of Lockyer, the numbers increasing with the temperature. The lines inclosed in parentheses coincide, to within errors of measurement, with air lines; they disappear also when the air lines disappear.

[1] Abridged translation of a paper appearing in *Comptes Rendus* (**139**, 188, July 18, 1904), sent by the author.

[2] *Comptes Rendus*, **134**, 1048, 1205, 1902.

[3] *Proc. R. S.*, **66**, 44, 1900.

[4] *Ibid.*, **67**, 403, 1901.

Lockyer	λ			Remarks
	α 6370.0	*P*	Strong.	Observed visually; should be sought in stellar spectra.
	α 6342.0	*P*	Strong.	
	β 5979.0	*D*	Easily visible.	
	β 5960.0	*D*	Easily visible.	
II	γ 5058.7	*P*	Strong.	Photographed on orthochromatic plates; should be sought in stellar spectra.
II	γ 5044.0	*P*	Strong.	
III	δ 4574.6	*D*	Distinct.	***Orion*** stars, ***β Crucis***, ***ε Canis Majoris;*** weak in ***α Cygni***.
III	δ 4567.5	*D*	Strong.	
III	δ 4552.3	*D*	Strong.	
II	ϵ 4131.0	*D*	Very strong, diffuse.	***Orion*** stars, ***Sirius***, ***Procyon***, ***Algol***, ***α Cygni***.
II	ϵ 4128.2	*D*	Very strong, diffuse.	
IV	(4116.5)	*D*	Short, very weak.	Stars of ***Orion***, and ***β Crucis***, where nitrogen lines have been recognized.
I	(4103.5)	*D*	Short, very weak.	Not found in stars of ***Orion***, or in eclipse spectra; Rowland gives *Si* 4103.1 in Sun and arc.
IV	(4007.3)	*D*	Short, very weak.	***Orion***, ***β Crucis***, ***γ Argus***, where nitrogen and oxygen lines have been found.
IV	(4089.3)	*D*	Weak, diffuse.	
I	ζ_1 3905.7	*P*	Very strong, sharp.	Seen in arc, spark, Sun, and eclipse spectra, ***Sirius***, ***Polaris***, ***Procyon***, ***Aldebaran***, ***Arcturus***.
II	ζ_2 3862.5	*D*	Strong, sharp.	*Orion* and ***α Cygni***.
II	ζ_2 3856.2	*D*	Strong, sharp.	
	ζ_2 3854.0	....	Short, weak.	
	η 3807.5	*D*	Rather st'g, sharp.	*ε Canis Majoris*.
	η 3796.0	*D*	Rather st'g, sharp.	
	η 3791.5	*D*	Easily visible, sharp.	*Orion* stars.

By comparison with recent stellar photographs, the following conclusions appear to be warranted:

1. Only stars belonging to the first class show those lines which disappear under self-induction. Helium stars, such as those of ***Orion*** or ϵ *Canis Majoris*, give those lines which are the first to disappear, as, for instance, the triplet *Si* δ. The hydrogen stars such as ***Sirius*** and those which approach the solar type, as *Procyon*, yield those lines which are the last to disappear, *Si* ϵ and *Si* ζ_2.

2. Stars of the solar type exhibit those persistent lines which are common to arc and spark, *e. g.*, *Si* ζ_1; they are observed also in the flash spectrum. It will be interesting to determine whether the groups *Si* α and *Si* γ are not also found in stars of this class.

3. Stars of the third and fourth classes—presumably of low temperature—do not give any silicon lines.

It may be added that those lines which correspond to Lockyer's Group IV and which, according to him, indicate high temperature, are always associated on my plates with air lines; they coincide with lines of oxygen

and nitrogen, elements which have already been detected in several stars of the *Orion* type and in *β Crucis.*[1] I therefore consider group IV due to air.

As to the more refrangible part of the silicon spectrum, where no coincidences have been observed in stellar spectra, the strong line at λ 2542 disappears when the inductance reaches 0.006 henry.

The other lines, notably the characteristic group of six lines just below λ 2500 and the group at λ 2217–2209 show no diminution with my largest inductance, 0.03 henry.

A. DE GRAMONT.

PARIS, FRANCE,
July 18, 1904.

THE CROCKER ECLIPSE EXPEDITIONS IN 1905.[2]

THE next observable total solar eclipse occurs on August 30, 1905. It is remarkably well situated and is looked forward to with great interest. The shadow path begins at sunrise south of Hudson's Bay, enters the Atlantic Ocean a short distance north of Newfoundland, crosses northeastern Spain, northeastern Algiers and northern Tunis, passes centrally over Assuan on the Nile, and ends at sunset in northeastern Arabia. The durations on the coast of Labrador, in Spain, and at Assuan are two and one-half, three and three-fourths, and two and three-fifths minutes, respectively.

The interval of two hours and one-half between the instants of totality in Labrador and Egypt offers an unusual advantage for obtaining large-scale photographs of the solar corona, with a view to determining changes in the forms and positions of the delicate details of structure. The opportunity to bring the search for intra-mercurial planets to a satisfactory conclusion is also exceedingly promising. Should a new planet be observed at three stations, the interest attaching to its discovery would be heightened by the fact that its approximate orbit could be determined at once. If no planets are revealed on good photographs, the negative results would be scarcely less valuable, though certainly less interesting, than positive results, and the intra-mercurial question would cease to be a pressing eclipse problem.

An observing station in Spain would contribute to both of the above investigations, as well as to many polarigraphic and spectrographic studies.

An important element in the success of eclipse observations consists

[1] MCCLEAN, *Spectra of Southern Stars* (London, 1898), and "Comparative Photographic Spectra," *Phil. Transactions* (1898).

[2] From *Lick Observatory Bulletin* No. 59.

in the opportunity to prepare the instrumental equipment and the program and methods of observation well in advance, in order that critical tests may be made before the expeditions depart for their observing stations.

It is a pleasure to announce that Mr. William H. Crocker has again shown his interest in the science of astronomy by offering to meet the expenses of expeditions to be sent from the Lick Observatory, University of California, to Labrador, Spain, and Egypt, to secure observations of the 1905 eclipse.

The provisional program for the three stations is, in the main, as follows:

LABRADOR.

A photographic search for intra-mercurial planets in a region of the sky 8½° wide, extending along the direction of the solar equator from 4° below the Sun to 15° above it.

The photography of the corona by means of a camera of five inches aperture and forty-foot focus, of the form first used by Professor Schaeberle at the eclipse of 1893.

SPAIN.

A photographic intra-mercurial search covering a region 9¼° wide, extending in the direction of the solar equator from 14° below to 14° above the Sun.

The photography of the solar corona with a camera of five inches aperture and forty-foot focus.

A polarigraphic study of the polarized light in the corona.

The use of spectrographs provided with moving plate-holders, to obtain continuous records of changes in the spectrum of the Sun's edge at the times of second and third contacts; of spectrographs for determining the wave-length of the green coronal bright line, and, if possible, the accurate wave-lengths of the bright and dark lines in the isolated spectrum of the Sun's edge, as nearly as possible at the time when the dark lines give way to bright ones, and *vice versa;* and of a spectrograph for recording the general spectrum of the corona.

EGYPT.

A photographic intra-mercurial search in a region 8½° wide, extending in the direction of the solar equator from 4° below to 15° above the Sun.

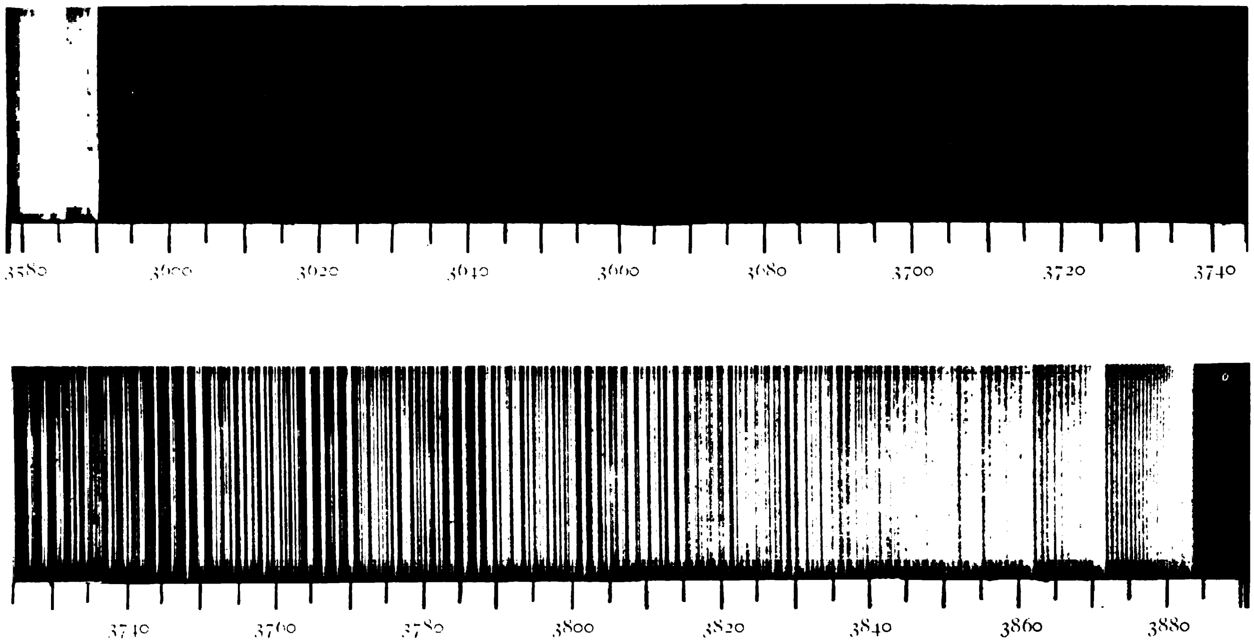
The photography of the solar corona with a camera of five inches aperture and forty-foot focus.

The photography of the general spectrum of the corona.

W. W. CAMPBELL.

MOUNT HAMILTON, CALIF.,
August 3, 1904.

PLATE XV.



THE THIRD CYANOGEN BAND.

THE
ASTROPHYSICAL JOURNAL
AN INTERNATIONAL REVIEW OF SPECTROSCOPY
AND ASTRONOMICAL PHYSICS

VOLUME XX NOVEMBER 1904 NUMBER 4

REGULARITIES IN THE STRUCTURE OF THE THIRD CYANOGEN BAND.[1]

By FRANZ JUNGBLUTH.

THE study of the band spectrum of carbon, as given by the electric arc in air between carbon terminals, has been productive of some important additions to our knowledge of banded spectra. A high dispersion shows each band of the carbon spectrum to be made up of several "heads," from each of which a series of fine lines proceeds toward the region of shorter wave-length.

Kayser and Runge[2] measured the fine lines of several bands as far as the resolution of their grating would allow, and noted some regularities in the positions of the lines. The band with its first head at λ 3884 seemed to them most interesting in its structure. This is one of the cyanogen bands, so called from their apparent dependence on the presence of nitrogen in the atmosphere, allowing a combination of this element with the carbon.

In the band beginning at λ 3884 we can see (Plate XV) that a series of lines starts from the first head, the separation of successive lines increasing as they recede from the head. A second head appears at λ 3872, and the superposition of its series on that of the first head gives a denser structure, but one can still select the strong lines of the first series which are contrasted with the weaker ones of the second. At

[1] Translated from the author's *Inaugural Dissertation, Universität zu Bonn.*

[2] "Ueber die im galvanischen Lichtbogen auftretenden Bandenspectra der Kohle," *Abhandl. d. Akad. d. Wissenschaften zu Berlin*, 1889.

λλ 3862, 3855, 3852 other heads occur, each adding its series of lines to those of the preceding heads, so that below λ 3852 the structure is very complicated, and it would be difficult to follow the several series if the lines of each did not retain a definite relative intensity, those belonging to the first head being strongest, and the intensity diminishing with each successive series. Further aid is derived from frequent coincidences of lines belonging to different series. When several lines coincide in this way to form one strong line, we see close to this a weak line, a little further a stronger one, then a still stronger and perhaps a fourth line stronger than any of the others (see Plate XV, at λλ 3837, 3801, 3768, 3705).

Kayser and Runge[1] were able to separate the lines of the first three series and follow them some distance from the head of each, studying the arrangement in each series. In this way they tested the law deduced by Deslandres[2] for the structure of band spectra, which states that the vibration-numbers of successive lines in a series form an arithmetical progression, and showed that it holds only for the beginning of each series in this band, and that beyond a certain point the successive lines lie much closer together than allowed by Deslandres's law.

A conception of the structure of bands quite different from that of Deslandres was arrived at by Thiele[3] after a close study of a number of spectra. Thiele advanced the hypothesis that the wave-lengths of the lines of a series are to be represented by the equation

$$\lambda = f\,[(n+c)^2],$$

where n passes through all values of whole numbers and c is a constant called the "phase" of the series. According to this equation, λ must have a maximum and a minimum value, corresponding to $\lambda_0 = f(0)$ and $\lambda_\infty = f(\infty)$. The physical significance of this is that each series has not only a head at λ_0, where there are a finite number of lines, and from which the series proceeds with increasing intervals between successive lines; but also a definite ending at λ_∞, in approach-

[1] *Loc. cit.*, § 9.

[2] "Loi de répartition des raies et des bandes, commune à plusieurs spectres de bandes," *Comptes Rendus*, **104**, 972–976, 1887; "Loi générale de répartition des raies dans les spectres de bandes," *ibid.*, **103**, 375–379, 1886.

[3] "On the Law of Spectral Series," ASTROPHYSICAL JOURNAL, **6**, 65–76, 1897; "Resolution into Series of the Third Band of the Carbon Band-Spectrum," *ibid.*, **8**, 1–27, 1898.

ing which the intervals between lines become steadily smaller, and at which an infinite number of lines lie crowded together. This place was named by Thiele the "tail" of the series, and in the carbon spectrum should show a shading toward the red. Thus while according to Deslandres, the arithmetical progression required constantly increasing intervals between successive lines of a series, the theory of Thiele says that these intervals reach a maximum at a certain distance from the head and then decrease toward the "tail."

A few years ago some work by King[1] lent probability to Thiele's hypothesis. King found that with long exposure photographs of the carbon spectrum showed a number of clearly defined heads which shaded toward the red, therefore in the opposite direction to the heads hitherto known. These appearances in the spectrum he considered to be the tails predicted by Thiele, and found simple numerical relations between their wave-lengths and those of the heads.

Professor Kayser[2] had attempted to verify Thiele's theory by means of the existing measurements for the carbon lines, but the measurements proved not sufficiently complete, though they showed that the interval between lines probably decreased after a certain point in a series, as had been already noted by Kayser and Runge.[3] It appeared that in order to show clearly whether the assumption of King is correct, and with it Thiele's hypothesis, it would be necessary to use a higher dispersion than that of Kayser and Runge and so to obtain more accurate measurements of the lines in the cyanogen band λ 3884, and with them investigate the regularities in each series. At the suggestion of Professor Kayser, I undertook the work along this line.

The photographs were made with the aid of a Rowland concave grating of 6.6 meters radius and 630 lines to the millimeter. The grating was mounted according to the plan of Abney,[4] the arrangement being described in detail by Konen.[5] In this arrangement the grating and plate-holder are stationary, while the carriage containing the slit may be moved around a half-circle. The advantage

[1] "Some New Peculiarities in the Structure of Cyanogen Bands," ASTROPHYSICAL JOURNAL, **14**, 323–330, 1901; translated in *Annalen der Physik*, (4) **7**, 791–800, 1902.

[2] *Handbuch der Spectroscopie*, **2**, chap. viii, § 304, 1902.

[3] *Loc. cit.*, § 4.

[4] *Philosophical Transactions*, **177**, II, 457–460, 1886; see H. KAYSER, *Handbuch der Spectroscopie*. I, § 450, 1900.

[5] "Ueber die Krupp'sche Gitteraufstellung im physikalischen Institut der Universität Bonn," *Zeitschrift für wissenchaftliche Photographie*, **1**, 1903.

of such a mounting is that it allows the photographing of higher orders of spectra. As source of light the violet center of the arc was used, between carbons as pure as could be obtained. In order to get sharp definition for the different parts of the cyanogen band, very different lengths of exposure were required. While the heads appeared distinctly with an exposure of 5 to 10 minutes, the "tails" and adjacent portions required two hours. Plate XV is taken from a two hours' exposure, the heads being thus overexposed. It was found by trial that the third-order spectrum gave sufficient dispersion to permit measurements of the desired accuracy. The plates were measured on a dividing engine constructed according to designs of Professor Kayser.[1] As normals the standard iron lines of Kayser[2] were used. The possible error of measurement for clearly defined lines amounted to 0.003 Ångström unit.

The use of high dispersion, though primarily to allow more accurate measurements, resulted in the observation of several peculiarities in the structure of the band which had not been previously noticed. In the part of the band between the first and second heads three series may be observed, which go out from the first head: first a very weak series, only a few of whose lines are faintly visible; then a series of strong double lines close together; and finally a series of single lines, likewise strong. The arrangement suggests that to this last series belongs the well-defined head at λ 3883.56, while the double-line series begins before this point, at about λ 3883.63, a phenomenon which has been noted in other groups of bands.[3] The intensity gradations in these two series are noteworthy. The double-line series is at first the strongest, but rapidly falls off in intensity and at λ 3838 is scarcely visible, while the series of single lines which is at first the weaker one and even in one place invisible (at λ 3875), rapidly develops and forms the principal series of the band, which Kayser and Runge were able to follow to λ 3640. This structure, also, has been noted in other band spectra.[4] Behind the second head, while the dense structure renders observations uncertain, there appear again to be two series leaving the head, but this time both of double lines. Near the other

[1] *Handbuch der Spectroscopie*, **1**, chap. v, § 567.

[2] "Normale aus dem Eisenspectrum," *Annalen der Physik*, (4) **3**; ASTROPHYSICAL JOURNAL, **13**, 320, 1901.

[3] H. KAYSER, *Handbuch d. Spect.*, **2**, 484.   [4] *Loc. cit.*

heads the confusion of lines is too great to permit the separation of series.

Following the series farther, another peculiarity appears, which, strange to say, has not been heretofore observed, though it comes out distinctly in the first-order spectrum. The lines become gradually broader as they recede from the head, and each finally separates into two lines when it has reached a breadth of about 0.07 t.-m. We have now *double lines* similar to those above and below the second head. As the series proceeds, the interval between components of the double lines increases to 0.09–0.1 t.-m., and then decreases until the components unite again to form one line. The lines of all the observed series show this behavior, so that in certain parts of the band structure, as for example, above and below λ 3700 (see Plate XV), we have only double lines. (It may here be remarked that the single sharp lines in the photograph are due to iron as an impurity of the carbons.) This phenomenon may be described by saying that the band consists of double lines, the components of which are sometimes superposed and sometimes separated. It is a noteworthy property and must be considered in forming any valid theory concerning the origin of spectra. The lines of the fourth cyanogen band at λ 3590 show the same peculiarity.

The double-line structure shows, however, an irregularity of such frequent occurrence that it may almost be considered a regularity. In some places where a double line should appear, this is wanting, but in its stead a stronger single line occurs, very near to the position that the missing double line should occupy. Such an interruption of the double lines appears in the interval between the first and second heads, at about λ 3873; and the phenomenon is met with a number of times in following the other series—these places being easily selected in the tabulation of series—appearing so distinctly as to exclude an error of observation caused by the superposition of several lines.

As material for the study of the structure, I have succeeded in separating four series of regularly placed lines by means of the intensity differences and the frequent coincidences of lines belonging to different series (see pp. 237 and 238). It was possible to follow only the strongest series direct to its head. This is the last of the three series mentioned as proceeding from the first head. The other series could not be followed with certainty quite to their heads, but it can neverthe-

less be definitely stated that the three other series belong in order of intensity to the second, third, and fourth heads. In some places what seemed to be a fifth series could be detected, but on account of the small intervals between its lines and the very frequent coincidences with stronger lines of other series, it could not be accurately followed. The same difficulty prevented the following of the weak fourth series over a part of its extent.

My object being to test the validity of Thiele's hypothesis, the task next undertaken was to follow the strongest series, which promised to remain longest visible, as far as possible, perhaps even to one of the "tails" observed by King. It was found impossible, however, to trace the lines of this series beyond λ 3640, which was the limit reached by Kayser and Runge, and for two reasons: first, because the lines of different series cease at this point to retain the difference in intensity which is so noticeable earlier in their course; and, second, because of the extremely dense structure in this region (about 10 lines to the tenth-meter). For the same reasons, the second series could not be followed beyond the point mentioned, while the third and fourth series became indistinguishable at about λ 3680.

My measurements follow:

Series I	Series II	Series III	Series IV	Series I	Series II	Series III	Series IV
3883.558				3876.000			
....				459			
....				75.913			
....				....			
246				74.752			
136				137			
82.000				73.507			
850				72.871			
646				182			
444				71.526	**3871.526**		
228				70.801	....		
011				059	....		
81.734				69.313	....		
456				68.542	....		
149				67.745	....		
80.809				66.942	....		
470				118	....		
106				65.282	....		
79.727				64.428	....		
330				63.524	....		
78.903				62.624	....		
455					....	**3861.846**	
77.984				61.661	....	....	
502				60.753	....	....	

Series I	Series II	Series III	Series IV	Series I	Series II	Series III	Series IV
3859.797							3833.018
58.823						3832.773	
	3858.031			3832.571			
57.812							32.189
	57.280				3831.983		
56.788						31.757	
	56.526						31.320
55.756	55.756			31.170			
	54.978				30.772	30.772	
			3854.849				30.486
54.691				29.754		29.754	
	54.184				29.553		29.553
53.623						28.644	
	53.352						28.565
52.527	52.527			28.320	28.320		
	51.655						27.773
51.421							695
	50.779					27.502	
50.294					27.086		
	49.880				014		
49.144				26.916			26.916
	48.972			827			827
	063					26.439	
47.970						325	
	47.098						25.909
46.786					25.819		819
	46.134				736		
45.576				25.455			
	45.153			350		25.350	
44.362						261	
	44.142						24.938
43.121	43.121						869
	42.084				24.533		
41.861					456		
	41.035	3841.035				24.226	
40.586						148	
		40.235		23.976			23.976
	39.955			887			887
39.299		39.299			23.240		
	38.870				169		
		38.470				23.086	
37.988						011	23.011
	37.759						22.904
		37.549		22.484			
			37.015	390 *			
36.645	36.645	36.645			21.980	21.980	21.980
			36.249		834	834	834
		35.677		20.962 *			20.962
	35.490		35.490	878			878
35.306						20.782	
		34.727	34.727			704	
	34.336				20.540		
33.949			33.949				19.928
		33.744					867
	33.170					19.607	

Series I	Series II	Series III	Series IV	Series I	Series II	Series III	Series IV
		3819.527			3807.835		
3819.428					746		3807.746
349							642
	3819.199					3807.017	
			3818.894			06.931	
			812	3806.599			06.599
		18.411		514			514
		332			06.343		
17.880	17.880		17.880		262		
17.803	17.803		17.803			05.678	
		17.201				592	
		115					05.345
			16.768				273
			684	04.934			
	16.429			842	04.842		
16.333	333				753		
228						04.323	
		15.984				242	
		895					04.140
			15.674				070
			604		03.330		
	15.049			03.240	240		
	14.960			148			
14.744		14.744				02.959	02.959
656		656				869	869
			14.573		*01.795		
			488		710		01.710
	13.634					01.626	626
	563			01.531		531	
		13.476	13.476	441			....
		302	392		00.249		
13.146					177	00.177	
058						120	
			12.340	3799.818			
	12.217	12.217	217	724			
	121	121				3798.792	....
11.534					3798.697	697	
446					600		
			11.208	98.083			
			128	97.993			
		10.941				97.385	....
		855				300	
	10.763				97.114		
	681				030		
			10.064	96.338			....
			09.993	247			
09.903						95.957	
824						875	
		09.644			95.530		
		555			447		
	09.310			94.560		94.569	....
	220			484		484	
			08.805		93.939		
			811		850		
08.200		08.200				93.080	

*The sign signifies that between them a dense group of lines form a dark ground.

Series I	Series II	Series III	Series IV	Series I	Series II	Series III	Series IV
		3792.996		3776.069			
3792.802			....		3775.426		
717					332		....
	3792.319					3774.731	
	230					642	
		91.622		74.253			
		538		163			
91.007					73.694		
90.917			....		606		....
	90.688					73.148	
	601					059	
		90.145	....	72.325			
		033		233			....
89.194					71.947		
116					855		
	89.037					71.556	
	88.952					457	
		88.675		70.397			
		588	....	292			
87.379	87.379				70.171		....
217	217				074		
		87.032				69.943	
		86.922	....			848	
	85.715			68.463			
	624			383	68.383		
85.535		85.535			300	68.300	
454		454				220	
	84.046	84.046	....				....
	83.943	83.943				66.698	
83.683					66.602	602	
593				66.474	474		
		82.530		369			
		444				65.068	
	82.353					64.980	
	255		....		64.798		....
81.833					705		
729				64.507			
		80.992		410			
		889				63.432	
	80.654		....			340	
	551				62.995		
79.956					900		
860				62.522			....
		79.445		431			
		350	....			61.787	
	78.948					680	
	854				61.170		
78.067					080		
77.973			....	60.523			
		77.885		435			
		795				*60.183	
	77.220					*037	....
		76.323	....		59.336		
		234			241		
76.156				*58.511		58.511	

Series I	Series II	Series III	Series IV	Series I	Series II	Series III	Series IV
*3758.366		3758.366				3739.707	
	3757.489					624	
							3739.158
	400						
		56.809					074
		731			3738.571		
56.492			....		490		
402						37.984	
	55.634					904	
				3737.822			
	546						
		....		748			
54.457							37.663
369			....				567
	53.773				36.644		
	53.684		....		561		
		53.343				36.171	36.171
							092
		253					
52.412				35.595			
323			....		34.711		34.711
					616		616
	51.902						
	810					34.440	
		51.679		33.577			
		593	....	489			
							33.206
50.357							
269							123
	50.035	50.035			32.785	32.785	
	49.924	49.924			690	690	
							31.722
			....				
							640
48.297		48.297					
206		206		31.441			
	48.129			355			
						31.082	
	049		....				
						010	
		46.583					
		498			30.821		
46.229	46.229				737		
132	132		....				30.244
							161
		44.875					
		787		29.295		29.295	
	*44.259			211		211	
44.128	128				28.870		
*048					783		28.783
			3743.581				694
			404			27.539	
		43.156				451	
		070					27.304
	42.408						229
	324			27.131			
42.043			42.043	053			
41.054			41.054		26.901		
		41.431			826		
		349				25.793	25.793
			40.635			719	719
	40.500		500	24.056	24.056		
	411			867	867		
39.939						24.057	....
854						23.980	

Series I	Series II	Series III	Series IV	Series I	Series II	Series III	Series IV
	3722.967					3708.451	
	886		3722.886			377	
3722.790			790				3707.690
715							622
		3722.314		3707.413			
		238		334			
			21.443		3707.098		
			370		028		
	20.996					06.740	
	919					672	
20.614		20.614					06.398
533		533					343
			20.016	*05.193			
			19.943	117	05.117		05.117
	19.041				037	05.037	037
		254				*04.965	
		18.838					03.887
		764					825
			18.602			03.336	
			522			03.270	
18.433					03.123		
349					049		
			17.195	02.959			
		17.110	110	895			
	17.024	024					02.619
	16.944					01.636	
16.243						574	
168							01.425
			15.804		01.123		
			734		063		
		15.372		00.738			....
		297		662			
	15.049					3699.958	
	14.974					894	
			14.419		3699.138		....
			348		080		
14.040				3698.500			
13.964				98.435			
		13.640				98.284	....
		568				220	
	13.067		13.067		97.130		
	12.987		12.987		064		....
		11.921				96.605	
11.836		836				557	
764				96.256			....
			11.710	197			
			661		95.149		
	11.077				090		....
	005					94.956	
			10.420	94.022			
09.628				93.958			
558		....				93.264	....
	09.096				93.172		
	016		09.016		113		
			08.922	91.776			

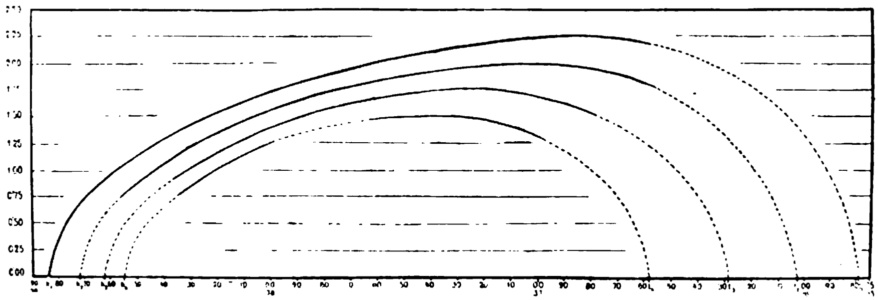
Series I	Series II	Series III	Series IV	Series I	Series II	Series III	Series IV
3691.716			....		3662.220		....
		3691.653				....	....
	3691.189				60.394		....
	132		....	3660.288		....	....
		90.034			58.563		....
89.532						....	**3685.275**
471			....	58.054			
	89.209				56.749	....	
	160			55.832			
		88.435	....		54.976		
87.254	87.254			53.617		....	
		86.849			53.234		
			....			....	
		85.310			51.522		
	85.252			51.417		....	
85.015					49.877		
		83.717	....	49.210		....	
	83.200				48.276		
82.761		....		47.031		....	
	81.333		....		46.757		
80.506						....	
	79.365	——.....			45.281	....	
78.252			....	44.828		....	
	77.428				....	....	
75.097		....	....	42.644		....	
	75.490				....	....	
73.741		....	....	40.449		....	
	73.563			....	....	....	
		....	....		....	....	
	71.030			....	....	....	
71.402		....	....		....	....	
	69.746			....	....	....	
69.241			....	....	....	**3628.984**	
	67.840	....		....	....		
66.997			....	....	....		
	65.054	....		....	**03.12†**		
64.708			....	....			
	64.000			**3579**			
62.522		....	....				

† Value by King.

As another means of tracing the series a graphic representation was tried, and this not only gave a better view of the course of each series, but allowed a comparison of the several series among themselves. As abscissæ I have used the distance of the lines from λ 3884, the beginning of the group of bands, and as ordinates a multiple of the interval between successive lines of a series, both expressed in wave-lengths. In the case of double lines the mean wave-length of the components was taken. By the use of these numbers the

full lines of the curves below were obtained. The dotted portions of the curves are hypothetical.

The first parts of the curves show nothing new. It is interesting, however, to note that a short distance below the heads the curves run parallel to each other at almost equal distances apart. Of far greater interest, however, is the fact that farther along we see in each series that the distance between successive lines increases up to a definite point, remains a short time constant, and then decreases—a peculiarity which Kayser and Runge thought they observed in the first series. *We therefore have here a confirmation of a part of Thiele's*



hypothesis. I may here mention another remarkable regularity: that *the maximum intervals for the several series form an arithmetical progression.* The approximate values of these intervals are as follows:

Series	Maximum Interval	Difference
I	2.25	
II	2.00	0.25
III	1.75	0.25
IV	1.50	0.25

The curves also demonstrate an essentially new property, in that they show that the *strongest* series, that from the *first* head, possesses the *greatest* maximum interval between lines and begins *latest* to decrease this interval; while the weakest series, proceeding from the *fourth* head, has the *smallest* maximum interval and begins to show decreasing intervals *earlier* than any other series. This fact points with tolerable certainty to the conclusion that the first series,

the strongest, extends the farthest, and that the last, the weakest, has the shortest course; in other words, that if the series end in tails at all, then the tail of the first series must lie farthest from, and that of the fourth series nearest to, their respective heads. It would at least be in contradiction of all our views of related physical conditions if the relation were otherwise; the more so as the difference in intensity as well as the unequal maximum intervals between lines of the different series give a measure of the energy possessed by the particle producing each.

This conclusion finds a supplementary confirmation in another phenomenon, previously unnoticed. If we look at the tails, which like the heads lie together in groups, we see that in such a group the tail of shortest wave-length is the strongest, while the other tails decrease in strength toward the red—the reverse of the behavior of the heads. This is to be seen distinctly in the three tails at $\lambda\lambda$ 3658, 3629, 3603 in Plate XV. The evidence is therefore strong that if a group of heads is connected with a group of tails, then ***the first head and the last tail belong to the same series, also the second head and the tail next to the last***, etc.

The recognition of this relation places the connections between heads and tails found by King in a new light. King (*loc. cit.*) divided the wave-length of the *first* head of a group by that of the *first* member of a group of tails, the *second* head by the *second* tail, and found simple numerical relations between these quotients for the several cyanogen bands. Since these connections, as has been shown, have nothing to do with the actual structure of a band, they indicate the more clearly that the different groups of heads and tails belong together as a whole. We therefore have here a ***new confirmation of King's view and also of the second part of Thiele's hypothesis.***

The question now arises: To which group of tails do the series of the third cyanogen band run—to the group beginning at λ 3658, or to that with the first tail at λ 3466? King inclined to the latter view, but this does not seem to me to be correct. If the strongest series has a symmetrical course, *i. e.*, if its curve descends as rapidly as it ascends, the first tail of this series would fall at λ 3466.[1] But then the tails for the other three series would be lacking, as these should lie above

[1] H. KAYSER, *Handbuch der Spectroscopie*, **2**, 486, 1902.

the first tail. It seems to me more probable that the series end in the group of tails λλ 3658, 3629, 3603. We must, indeed, then assume that the intervals between successive lines decrease much faster than they increase. It appears as if this were in fact the case, but as it was in no case possible to follow a series for more than a few lines beyond its position of maximum interval, no positive statement can be made.

Assuming that the groups of heads and tails are so related, we have yet the question: How are the separate heads and tails to be combined? The simplest assumption would be that the first series runs to the tail at λ 3603, the second to λ 3629, and the third to λ 3658; then we must further assume that the tail of the fourth series is too weak for observation. I wish, however, to call attention to another possible combination which leads to quite peculiar relations.

As the intensity of the members of a group of tails increases toward the ultra-violet, it seemed worth while to see if another, stronger tail could be found below the tail at λ 3603. If this looked-for tail has a regular position in the group, it should lie at about λ 3579; but it is obviously difficult to decide whether a tail is present at this place, which is just below the strong first head of the cyanogen band at λ 3590. However, we are inclined to make this assumption, particularly as it may at the same time account for the hitherto unexplained intensity-minimum below λ 3590. Then we must assume that the principal series runs to this tail, while the second goes to λ 3603, the third to λ 3629, and the fourth to λ 3658; and here I wish to point out some simple relations which exist between the heads and tails when so combined. If we subtract the wave-length of each tail from that of its head, we obtain:[1]

Series	Length of the Series	Difference
I.	3884 − 3579 = 305	
II.	3872 − 3603 = 269	36
III.	3862 − 3629 = 233	36
IV.	3855 − 3658 = 197	36

It seems, then, that *the lengths of the successive series form an arithmetical progression.*

[1] In this and the following table approximate wave-lengths are used, since the wave-lengths of the tails at λ 3579 and λ 3603 could not be accurately determined.

Further, if we divide each head by its tail, we get the following:

Series	Quotients	Difference
I..........	$\frac{3884}{3579}=1.0852$	
		0.0105
II..........	$\frac{3872}{3603}=1.0747$	
		0.0105
III..........	$\frac{3862}{3629}=1.0642$	
		0.0103
IV..........	$\frac{3855}{3658}=1.0539$	

We see, therefore, that also *these quotients form approximately an arithmetical progression.*

It remains of course doubtful whether in this assignment of heads and tails the real structure of the band has been found. At any rate, these relations demonstrate anew *the correctness of King's view of the band structure.* It would be a profitable task to determine by the aid of my measurements and the formula given by Thiele which combination of heads and tails is the correct one.

The chief results of the foregoing work may be summarized as follows:

1. *The third cyanogen band consists of double lines* whose components lie close together and sometimes superposed.

2. *The maximum intervals between successive lines in the four strongest series form an arithmetical progression.*

3. *The view of King*, that the inverted heads found by him are to be regarded as tails of bands connected with the known heads, *possesses a high degree of probability.*

4. *The connection of groups of heads and tails is such that the first head and the last tail belong to the same series, the second head and the tail next to the last*, etc.

5. *The hypothesis of Thiele*, according to which the intervals between successive lines of a series increase only to a certain point, and then decrease until the series ends in a tail, *appears to be correct.*

I take pleasure here in acknowledging my obligation to Professor Kayser, who suggested this work and has followed its progress with constant interest. I wish also to thank Dr. Konen and Dr. Hagenbach for the counsel and assistance which they have given me in the experimental part of the work.

Physical Institute, University of Bonn,
May 1904.

THE SPECTRA OF SUN-SPOTS IN THE RED AND YELLOW REGIONS OF THE SPECTRUM.

By A. L. Cortie, S. J.

The present paper contains a reduction of all the observations of the Sun-spot spectra in the red and yellow regions of the spectrum, taken at Stonyhurst during the years 1883–1901. The spectroscope used was an automatic twelve-prism instrument by Browning, each prism having a refracting angle of 60°. The instrument was in the earlier observations attached to the eight-inch equatorial, and since the year 1894 to the fifteen-inch Perry memorial equatorial. The method of observation is first to pass the spectrum between B and D in review so as to pick out the most widened lines, and then to make a detailed study of some particular portion of this region. The observations are very tedious, and to secure a careful scrutiny of each line in the whole region would occupy between three and four hours. The slit is placed parallel to the diurnal motion, so that the spot remains on the slit even when there are slight irregularities in the driving of the clock.

A full discussion of the observations of the spectra of ninety spots made according to this plan in the years 1883–89 was published in Volume **50** of the *Memoirs of the R. A. S.*, and the reader is referred to that paper for the sections dealing with the behavior of the metallic lines in the spectra of Sun-spots, the lines of unknown origin in the spectra of Sun-spots, the C and D_3 lines in the spectra of Sun-spots, as well as various observations on the spectra of Sun-spots, though it must be noticed that most of the faint lines widened in the spectra of Sun-spots, then of unknown origin, have since been identified as due to vanadium and titanium. But Section 1 of that paper, which contained the list of lines between B and D widened in Sun-spots, was based upon the Ångström wave-length numbers, as revised and corrected in the British Association *Catalogue of the Oscillation-Frequencies of Solar Rays* (1878), supplemented by wave-lengths

taken from the maps of Fievez and Piazzi Smythe. Many of the lines then unidentified have since been identified, and the whole catalogue has been reduced to Rowland's wave-lengths contained in his *Preliminary Table of Solar Spectrum Wave-Lengths* (1898), much help having been derived in the reduction from Mr. Higgs's beautiful maps of the solar spectrum. The wave-lengths differ by about one unit from Ångström's as corrected.

Since the publication of the *Memoir* the observations have been somewhat intermittent, so that only twenty-four additional spots have been observed in the period 1890–1901, the results obtained having appeared in a series of papers in the *Monthly Notices, R. A. S.*, and a catalogue having been published in Volume **63**, 469, 1903. All the former lists have now been combined in the present catalogue of 349 lines between wave-lengths 5884.03 and 6867.46 in which Rowland's numbers have been used. The total number of individual observations it contains is 5486, the greatest number of spots in which any single line has been observed being 99, and the least 1. One chief object of the reduction is to enable other observers to supplement the observations in parts of the region where comparatively few spots have been observed. Some of the earlier observations were taken in conjunction with the late Father Perry, otherwise all the observations have been taken by the same observer with the same instrument. In the following list of lines the first, second, and fifth columns are taken from Rowland's table; the third contains the number of spots in which the several lines were observed, each separate daily observation even of the same spot being counted; and the fourth, the mean relative widening of each line. The manner in which the numbers in this column have been derived is as follows: In each observation of a widened line the amount of widening is estimated approximately in terms of the tenth of the normal width of the line, and the mean derived from these separate observations has been multiplied by 10. A final column contains remarks upon the lines.

TABLE I.

Lines between D and B widened in the Spectra of Sun-spots.

Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
5857.67	*Ca*	11	3.0	8	Observations made in 1884.
59.81	*Fe*	3	5.0	5	Observations made in 1884.
62.58	*Fe*	10	3.0	6	Observations made in 1884.
66.68	*Ti*	11	3.0	3	Observations made in 1884.
84.03	*Fe*	1	2.0	4	Observations made in 1884.
* 90.19	D_2, *Na*	99	4.0	30	For discussion on behavior of D lines in spots, see *Memoirs R. A. S.*, **50**, 47, and *Monthly Notices*, **63**, 478, 1903.
91.72 }	A (wv)	18	4.0	0	Once obliterated over spot.
91.88 }				4	
93.10	*Ni*	43	3.0	4	Darkened twice; less dark once.
* 96.16	D_1, *Na*	99	4.0	20	
99.52	*Ti*	9	4.0	1	
5900.14 }	A (wv)	1	3.0	2	
00.26 }	A (wv)			4	
05.90	*Fe*	10	3.0	4	
10.99	A (wv)	3	6.0	2	
* 14.34	*Fe*	13	2.0	4	Darkened.
15.65	A (wv)	9	6.0	1	Darkened; once much widened in penumbra.
18.64 }	A (wv)			4	
19.28 }	A (wv)	11	6.0	5	
19.86 }	A (wv)			7	
22.74	A (wv)	9	6.0	2	
24.04 }	A (wv)	10	4.0	2	
25.22 }	A (wv)			2	
28.01	*Fe*	15	4.0	2	
30.41	*Fe*	16	3.0	6	
32.31	A (wv)	13	3.0	5	
32.99	A (wv)	13	3.0	2	
34.88	*Fe*	16	3.0	5	
36.04	A (wv)	9	4.0	0	
38.27	A (wv)	16	7.0	0	High Sun line; very much darkened.
41.29	A (wv)	13	3.0	5	
41.85	A (wv)	14	4.0	2	
42.79	A (wv)	14	3.0	3	
44.95	A (wv)	14	4.0	1	Darkened.
45.87	A (wv)	14	3.0	1	Darkened.
46.22	A (wv)	15	3.0	3	Darkened.
48.77	*Si*	16	3.0	6	
49.57	*Fe*	16	2.0	1	Darkened.
51.72	A (wv)	2	7.0	0	
52.94	*Fe*	20	3.0	4	
53.39	*Ti*	2	4.0	1	
56.92	*Fe*	16	3.0	4	
58.10 }	A (wv)	20	3.0	1	
58.46 }	A (wv)			1	

* Lines marked with an asterisk are lines observed in the chromosphere by PROFESSOR YOUNG (SCHEINER'S *Astronomical Spectroscopy*, Frost's edition, p. 423).

TABLE I—*Continued.*

	Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
	5958.84		1	5.0	1	
	66.06	*Ti*	23	6.0	2	Much darkened.
	68.50	A (wv)	24	4.0	2	
	71.56	A (wv)	13	6.0	1	
	75.58	*Fe*	26	3.0	3	
	77.01	*Fe*	26	2.0	4	
	78.77	*Ti*	33	11.0	1	Widened often far into penumbra.
	....		1	10.0	....	Faint spot line.
	83.01	*Fe*	27	2.0	5	
	85.04	*Fe*	27	2.0	6	
	87.29	*Fe*	23	3.0	5	
	89.51	A (wv)	11	7.0	0	Darkened; once unaffected.
*	91.60		15	3.0	2	Darkened.
	96.96	*Ni*	7	4.0	1	
	98.00	*Fe*	25	2.0	2	Darkened.
	99.92	*Ti*	23	10.0	0	Always much widened.
	6003.24	*Fe*	29	3.0	6	
	05.77	*Fe*	21	9.0	1	
	08.10	*Fe*	31	3.0	4	
	08.70	*Fe*			6	
	12.45	*Ni*	17	5.0	1	Darkened; unaffected once.
	13.72	*Mn*	35	4.0	6	
	16.86	*Mn*	35	4.0	6	
	18.52		2	10.0	0	
	20.23		..	....	2	Difficult to separate.
*	20.40	*Fe*	32	4.0	4	
*	22.02	*Mn*	33	4.0	6	
*	24.28	*Fe*	34	3.0	7	
*	27.27	*Fe*	34	3.0	4	
	30.11	A (wv)	1	5.0	0	
	31.24	....	1	5.0	00	
	34.27	A ?	2	8.0	0	
	35.58	A ?	2	8.0	0	
	36.60	A ?	2	8.0	0	
	39.95	*V*	42	10.0	0	Always much widened; darkened.
*	42.32	*Fe*	26	2.0	3	Darkened.
	52.90 ?	....	1	20.0	00	
	53.91	*Ni*	43	8.0	0	
	56.23	*Fe*	39	8.0	5	Darkened.
	57.48	....	7	14.0	00	
	59.20 ?	....	1	30.0	....	Seen once.
	63.08	....	42	9.0	0	Much widened always.
	64.85	*Ti*	4	14.0	00	Darkened.
*	65.71	*Fe*	43	3.0	7	
	77.12	....	9	8.0	00	
	78.71	*Fe*	47	3.0	5	Darkened.
	79.23	*Fe*			2	
	81.67	*V*	21	7.0	0	
	82.93	*Fe*	23	3.0	1	
*?	85.47	*Ti, Fe*	22	3.0	2	
	86.50	*Ni*	15	3.0	1	
	88.05	....	25	5.0	00	

TABLE I—*Continued.*

Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
6089.79	*Fe*	18	4.0	1	Darkened.
90.43	*Ti, V*	1	10.0	2	
91.40	....	18	6.0	0	Obliterated once.
93.86	*Fe*	12	4.0	2	Obliterated once.
94.59	*Fe*	12	2.0	1	
96.88	*Fe*	15	3.0	3	
98.47	....	15	4.0	0	
6100.49	....	18	4.0	00	Obliterated once.
* 02.39	*Fe*			2	Three lines difficult to separate in spots; the greater part of the widening due to the *Ca* line; displaced once.
* 02.94	*Ca*	87	5.0	9	
* 03.40	*Fe*			4	
05.35	....	12	7.0	0	
08.33	*Ni*	22	4.0	6	
* 11.29	*Ni*	20	5.0	2	
16.40	*Ni*	22	4.0	4	
19.74	*V*	20	5.0	1	
19.97	*Ni*			0	
* 22.43	*Ca*	83	5.0	10	Displaced once; reversed once.
25.24	....	13	3.0	1	
26.44	*Ti*	15	1.0	1	Darkened; displaced to red.
28.12	*Fe*	19	5.0	3	Less intense once.
29.19	*Ni*	5	4.0	1	
30.34	*Ni*	2	0.0	1	
31.79	....	15	5.0	0	
35.58	*V*	9	13.0	00	Very black once.
* 36.83	*Fe*	74	4.0	8	Unaffected once; displaced to violet.
37.21	*Fe*			3	
37.92	*Fe*	59	4.0	7	Unaffected once; displaced to violet.
38.73?	....	1	10.0	00	
* 41.04	*Fe, Ba*	62	2.0	7	Unaffected six times; displaced to violet.
42.70	....	1	0.0	1	Obliterated once.
45.23	....	14	5.0	2	Obliterated once.
47.95	....		3.0	2	Unaffected once.
* 48.04	*Fe*	7		3	
* 49.46	....	13	8.0	2	Widening probably due to the near *V* line.
50.36	*V*	4	9.0	0	Band at this position once.
51.05	....	2	15.0	0000	Very faint double.
51.55				0000	
51.83	*Fe*	13	5.0	4	Darkened once.
* 54.44	*Na*	16	9.0	2	Displaced to violet once.
55.35	....	1	0.0	7	Less dark over spot once.
56.24	....	1	2.0	00	Obliterated in spot once.
57.95	*Fe*	15	4.0	5	
59.59	....	1	3.0	0	
* 60.96	*Na*	41	9.0	3	Frequently surrounded in spots by a hazy fringe.
61.50	*Ca*	..	....	4	
* 62.30	*Ca*	59	3.0	15	Darkened; unaffected once. Displaced to violet once.

TABLE I—*Continued.*

Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
6163.77	*Fe*	55	3.0	1	
63.97	*Ca*			3	
65.02 ?	....	20	5.0	0000	Obliterated once in spot.
65.58	*Fe*	4	5.0	3	Unaffected once; less dark over spot once.
66.65	*Ca*	45	7.0	5	Darkened; widened far into penumbra.
69.25	*Ca*	5	8.0	6	Darkened.
69.78	*Ca*	59	4.0	7	Darkened.
70.73	*Fe, Ni*	57	4.0	6	Darkened.
* 73.55	*Fe*	18	4.0	5	Darkened.
* 75.58	*Ni*	18	5.0	3	Darkened; unaffected once.
* 77.03	*Ni*	16	4.0	5	Darkened; unaffected once.
80.42	*Fe*	18	4.0	5	Darkened.
83.78 ?	A (wv)	4	10.0	0	
85.02	*Fe*	15	5.0	1	
86.93	*Ni*	3	5.0	2	Unaffected once.
88.21	*Fe*	19	5.0	4	
90.61		2	9.0	000	A faint double.
90.87				0000	
91.30	*Ni*	40	2.0	6	
* 91.78	*Fe*			9	Darkened; unaffected three times.
96.43		2	5.0	0	
95.03		2	4.0	0	Unaffected once.
99.40	*V*	26	14.0	0	Very dark in spots twice.
99.98		2	10.0	0000	
*6200.53	*Fe*	24	4.0	6	
04.83	*Ni*	6	10.0	1	Darkened.
10.90		12	12.0	00	
12.28		3	20.0	00	Faint double.
12.48				0000	
13.64	*Fe*	24	4.0	6	
* 15.36	*Fe*	23	5.0	5	Darkened; displaced to red twice.
* 16.57		21	6.0	1	Attributed to *V* in Young's list of chromospheric lines. Darkened.
* 19.49	*Fe*	16	4.0	6	
* 21.01	*Fe*	9	8.0	0	
* 21.55	*Fe*			00	
24.20	*Ni* ?	13	6.0	1	
24.72	*V*	1	10.0	000	
26.95	*Fe*	16	7.0	1	Obliterated over spot once.
29.44	*Fe*	8	5.0	1	Obliterated over spot twice.
* 30.94	*Fe, V*	20	4.0	8	In the arc spectrum of *Fe;* displaced to violet once.
* 32.86	*Fe*	22	5.0	3	Displaced to violet once.
33.72		1	10.0	0000	
37.53		20	2.0	3	Unaffected three times; obliterated once.
* 38.60		19	1.0	2	Unaffected three times; obliterated once.
40.17		5	10.0	00	
40.53		4	9.0	00	Thalén: spark vanadium line 40.7.

TABLE I—*Continued.*

Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
6240.86	*Fe*	23	7.0	3	
43.06	*V*	45	28.0	000	Always much widened, especially the more refrangible line; most widened of all the lines.
43.32	*V*			1	
44.03		4	1.0	2	Unaffected three times.
44.69		4	1.0	2	Unaffected three times.
46.54	*Fe*	25	4.0	8	Unaffected three times.
* 47.77		5	5.0	2	Unaffected three times.
52.05	*V*	7	9.0	00	
52.77	*Fe*	30	6.0	7	Displaced to violet once.
54.38		9	4.0	1	Displaced to violet once.
54.46	*Fe*	35	5.0	5	Displaced to violet once.
56.57	*Ni, Fe*	34	6.0	6	Displaced to violet once.
58.32	*Ti*	8	8.0	2	
58.57	*V*			0000	
58.93	*Ti*	32	7.0	3	Displaced to violet once.
59.80		1	20.0	00	
61.32	*Ti*	29	9.0	1	Displaced to violet once; extended far into penumbra.
63.68 ?		2	4.0		
65.35	*Fe*	23	4.0	5	Displaced to violet once.
66.55	*V ?*	4	21.0	000	
67.04		4	8.0	0000	
69.08	*V*	7	14.0	000	Obliterated once.
70.44	*Fe*	14	3.0	3	Darkened; unaffected once.
71.49	*Fe*	12	3.0	0	Darkened; unaffected three times.
74.87	A (wv)	11	12.0	00	Fuzzy lines in spots.
75.48				000	
78.30	*O*	10	3.0	4	Head of the Alpha group.
79.08	*O*	1	8.0	2	
79.31	*O*			3	
79.95		2	5.0	0	Unaffected once.
80.11	*O*			0	
80.60	*O*	4	5.0	2	
80.83	*Fe*	10	5.0	3	
82.03	*Co, O*	10	6.0	2	Obliterated once.
86.03	A (wv)	7	8.0	00	Vanadium 6285.38; hazy over the spot once.
86.36				0	
87.95	*O*	5	8.0	1	
91.18	*Fe*	12	3.0	4	
92.38 ?	*O*	13	4.0	2	Vanadium 93.03; darkened once.
95.39	*O*	6	3.0	3	Hazy over spots twice.
96.17	*O*	4	3.0	3	
98.01	*Fe*	11	4.0	5	
99.44	*Fe, O*	14	4.0	3	Hazy over spots twice.
*6301.72	*Fe*	7	5.0	7	Displaced to violet once.
02.21	*O*	13	1.0	2	Unaffected twice; darkened.
* 02.71	*Fe*	13	2.0	5	
02.98	*O*	1	6.0	2	
06.02	*O*	18	16.0	2	Generally much widened; widening on violet side once; once extended into penumbra; a band near here.
06.78	*O*	6	3.0	2	Generally unaffected.

TABLE I—*Continued.*

Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
6310.10	*O*	2	4.0	2	Darkened.
10.85	*O*	12	6.0	1	Darkened.
11.72	*Fe*	2	3.0	1	Unaffected once.
12.46		1	10.0	00	
12.98		1	10.0	000	
14.88	*Ni*	4	6.0	4	Darkened.
15.19	*O*			0	
15.52	*Fe*	9	4.0	2	Darkened.
* 18.24	*Fe*	10	4.0	6	Darkened.
19.46		8	2.0	0	Darkened.
22.91	*Fe*	13	4.0	4	Darkened.
24.71	A (O)	5	5.0	00	Darkened.
25.75		3	9.0	0000	
27.82	*Ni*	2	4.0	2	
30.32	*Cr*	2	13.0	1	
31.07	*Fe*	2	5.0	2	Unaffected once.
32.18		1	8.0	0	
* 35.55	*Fe*	15	4.0	6	Darkened.
* 37.05	*Fe*	15	4.0	7	Darkened.
39.10	*Fe*	15	3.0	2	Darkened.
39.34	*Ni*			2	Darkened.
42.60	A (wv)	2	10.0	0000	Hazy over spot once.
44.37	*Fe*	13	5.0	4	
* 47.31		12	4.0	2	Unaffected once.
50.02	A (wv)	2 ?	9.0	0000	
55.25	*Fe*	12	3.0	4	
58.90	*Fe*	13	3.0	6	
61.03	*Ni*	2	8.0	0	Hazy over spot once.
61.42		2	0.0	0000	Hazy over spot once; unaffected once.
62.56	*Zn*	8	5.0	1	Variable line.
63.09	*Fe, Ca*	1	10.0	2	
64.58	*Fe*	1	8.0	0	
64.92				0	
66.71	*Ni*	5	1.0	0	
* 69.08	*Fe*	4	4.0	0	
* 71.57	*Fe*	1	5.0	1	
78.47	*Ni*	10	5.0	2	
80.96	*Fe*	13	6.0	4	Spot-band position: unaffected once.
83.93		6	9.0	0	
84.87		7	6.0	1	
85.95		2	6.0	0	
88.63		1	8.0	000	Spot-band position.
91.47 ?		1	8.0		Spot-line.
92.75		1	10.0	0	
* 93.82	*Fe*	50	3.0	7	Darkened: almost reversed once.
.....		3	8.0		Spot-line.
.....		2	8.0		Spot-line.
*6400.22	*Fe*	40	4.0	8	Darkened; weakened once; almost reversed once.
00.54	*Fe*			2	
05.98		10	9.0	00	
07.52		7	9.0	0	
08.23	*Fe*	41	4.0	5	Darkened.
11.87	*Fe*	41	4.0	7	Darkened.

TABLE I—*Continued.*

Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
.....		2	10.0		Spot-line; darkened.
6415.20		27	4.0	1	Darkened; less dark twice.
* 17.13	*Fe?*	27	4.0	1	Obliterated six times.
20.17	*Fe*	37	3.0	4	
21.57 }	*Fe*	38	3.0	7	
21.74 }	*Ni*			000	
25.08	....	3	7.0	00	
26.51	....	3	8.0	000	
31.07	*Fe*	37	4.0	5	Darkened.
* 32.90	*Fe?*	19	4.0	1	Unaffected three times. Not in Kayser and Runge's list of arc lines of *Fe*.
36.63	*Fe*	1	8.0	0	
39.29	*Ca*	47	5.0	8	Darkened.
49.36	....	1	5.0	0	
50.03 }	*Ca*			6	Close triplet in spots.
50.40 }	*Ca*	44	6.0	0	
50.55 }	*Co*			0	
51.79?	....	1	5.0	000	
52.54?	....	6	12.0	00	
54.35?	A	2	8.0	0000	Darkened.
55.23 }	*Co*	35	5.0	0	Darkened.
55.82 }	*Ca*			2	
* 56.60	....	7	0.0	3	Darkened, otherwise unaffected.
59.12	A	11	5.0	000	
59.90	A	2	7.0	0000	
* 62.78 }	*Ca*	47	6.0	5	Darkened; displaced to violet once.
62.97 }	*Fe*			3	Young doubts which component reversed. Arc line of *Fe* at 62.95 (Kayser and Runge).
63.72?	A (wv)	6	11.0	000	
64.90	....	8	7.0	00	
66.49?	A	2	6.0	000	
68.12	A	7	3.0	000	Displaced to violet once.
....	....	1	8.0	....	Faint spot line.
69.41	....	18	5.0	2	Darkened; displaced to violet once.
71.89	*Ca*	18	4.0	5	Darkened; displaced to violet once.
72.70 }	A (wv)	16	4.0	00	Darkened; displaced to violet once.
73.41 }	A (wv)			00	
75.44	A (wv)	3	3.0	0	
75.85	....	31	3.0	2	
79.41	A (wv)	3	9.0	00	
80.29	A (wv)	11	5.0	1	
82.10	....	10	3.0	3	
83.03 }	*Ni*	26	3.0	1	
83.47 }	A (wv)			1	
87.01	A (wv)	3	4.0	0	
91.88	*Mn*	20	4.0	1	Darkened.
94.00	*Ca*	23	8.0	6	Very dark once.
* 95.21	*Fe*	38	5.0	8	Very dark once.
96.08	A (wv)	22	2.0	1	
* 97.13	*Fe*	13	4.0	4	
99.88	*Ca*	20	6.0	4	
6508.83?	A (wv)	1	6.0	0	

TABLE I—*Concluded.*

Wave-Length	Origin	Number of Spots	Relative Mean Wid'ng	Intensity	Remarks
6512.24	A (wv)	4	7.0	00	
14.96	A (wv)	1	5.0	2	
* 16.31	....	27	4.0	1	
16.86	A (wv)	2	5.0	2	
17.32	A (wv)	1	5.0	0	
18.60	*Fe?*	3	7.0	2	
32.60	A (wv)	12	3.0	1	
44.14	A (wv)	19	2.0	2	Darkened.
46.48	*Ti, Fe*	6	4.0	6	
52.87	A (wv)	6	6.0	1	
59.82	....	1	4.0	0	
63.06	*H*	66	0.0	40	C line; generally either reversed or less dark over spots.
64.45	A (wv)	1	5.0	0	
69.46	*Fe*	29	4.0	5	
72.23	A (wv)	2	9.0	1	
73.03	*Ca?*	30	8.0	1	
75.27	*Fe*	43	3.0	2	Darkened.
81.45	....	25	5.0	0	
86.55	*Ni*	28	5.0	1	
91.58?	*Fe?*	1	3.0	00	
93.16	*Fe*	26	3.0	6	
94.12	*Fe*	35	2.0	4	Darkened.
98.85	*Fe?*	18	5.0	0	
6604.84	....	23	6.0	1	
08.28	....	9	3.0	0	
09.36	....	1	8.0	3	
25.28	....	7	6.0	0	
....	....	1	6.0	....	Spot line.
27.80	*Fe*	7	6.0	0	
33.09	*Fe*	37	2.0	2	
40.00	....	11	6.0	0	
43.88	*Ni*	37	2.0	5	Darkened.
54.15	*?*	1	8.0	00	
63.70	*Fe*	37	2.0	3	Darkened.
* 78.24	*Fe*	38	3.0	5	Chromospheric line 6678.3 (Young); very dark three times; weakened once over a spot.
6703.82	....	14	2.0	1	Darkened.
05.35	....	13	3.0	1	Darkened.
17.94	*Ca*	25	3.0	5	Darkened; once very dark; displaced to violet once.
22.10	....	3	5.0	2	Darkened.
26.93	*Fe*	14	4.0	2	
55.86	?	1	5.0	0	
68.03?	*Ni*	3	3.0	4	
87.10?	*Fe*	4	4.0	1	
6820.63	*Fe*	2	5.0	2	
28.85	*Fe*	1	2.0	2	
43.91	*Fe*	11	5.0	3	Very dark three times.
55.42	*Fe*	13	4.0	3	
67.46	A (O)	11	2.0	6	Head of B group; twice darkened.

The intensities of the lines are taken also from Rowland's table, in which a line marked 1 signifies one that is just clearly visible on his map of the solar spectrum, while successive zeros indicate successive degrees of faintness.

The chief phenomena in the spectra of Sun-spots are, as regards the general absorption; a want of uniformity in blackness in various regions of the spectrum, with parts sometimes obscured, and, as regards the selective line absorption; the widening of the lines, the darkening of lines without widening, displacement of lines, obliteration of lines, extension of the widening across the penumbræ of spots, reversal of lines, thinning of lines or reduction of intensity, hazy fringes to lines, especially the sodium lines, and spot-bands. Tables II and III contain the mean relative widening of all the lines of each element, and a list of lines with the greatest mean widening.

TABLE II.

Relative Widening of the Lines of Each Element.

Element	Total Number		Relative Mean	
	Lines	Observations	Widening	Intensity
Vanadium	11	186	12.3	0.0
Titanium	13	207	7.7	1.5
Calcium	18	734	5.1	6.3
Sodium	4	255	5.1	13.6
Nickel	28	566	4.5	2.2
Manganese	4	123	3.8	4.8
Iron	123	2599	3.7	3.9
Iron (chromospheric) lines	35	863	3.6	4.6
Oxygen ?	15	90	6.4	1.7
Water Vapor	47	364	4.3	0.8

The numbers for relative mean widening are derived in the same manner as in Table I. The tables show the important part played by the faint lines of vanadium and titanium in the spectra of Sun-spots, as was pointed out in 1898.[1] Lines which in the earlier observations were classed as of unknown origin have since been found to be due to vanadium or titanium. These faint lines are always at all times of the Sun-spot period among the most widened lines, λ 6243.06

[1] *Monthly Notices, R. A. S.*, **58**, 370, 1898.

TABLE III.

List of Lines with the Greatest Mean Widening.

Wave-Length	Origin	Spots	Relative Mean Widening	Intensity
5978.77	*Ti*	33	11	1
99.92	*Ti*	23	10	0
6005.77	*Fe*	21	9	1
39.05	*V*	42	10	0
6126.44	*Ti*	15	10	1
54.44	*Na*	16	9	2
60.96 }	*Ca*	41	9	3
61.50 }	*Na*			4
99.40	*V*	26	14	0
6210.90	....	12	12	00
43.06	*V*	45	28	000
61.32	*Ti*	29	9	1
74.87	....	11	12	00
6306.02	*O*	18	16	2
6405.98	....	10	9	00

of vanadium being particularly noticeable.[1] My observations afford no evidence of crossing points when faint lines of vanadium and titanium give way to lines of iron at a period between the Sun-spot maximum and minimum. They do, however, confirm the fact that the iron lines, while not displacing other faint lines, are more affected in minimum than in maximum spots.[2] This fact nevertheless would seem to give no warranty for any conclusion drawn from it as to an essential difference of character or temperature between maximum and minimum spots. A difference of level might possibly account for the greater relative importance of iron lines in minimum spots.

The iron lines which are bright in the chromosphere in this part of the spectrum are not affected differently in Sun-spots from lines not brightened in the chromosphere. These bright chromospheric lines are mainly arc lines.

The widening of some oxygen lines in Sun-spots, particularly in the *a* band, seems to be a real phenomenon. The single hydrogen line, the C line, is generally thinned and almost reversed over spots, and very frequently reversed and distorted in their immediate neighborhood. If oxygen and hydrogen are present in Sun-spots, it is conceivable that such a reduction of temperature might occur in

[1] *Ibid.*, **49**, 410, 1889, and **62**, 516, 1902.

[2] *Journal British Astronomical Association*, **1**, 175, January 1891.

their materials when they are ejected to considerable distances from the solar surface as to permit of their combination, and the formation of water vapor. Spot-bands which are sometimes seen in the spectra of Sun-spots would also, if they represent, as seems likely, the spectrum of a compound, witness to a great reduction of temperature in the materials constituting the spot.[1] But the widened lines accredited to water vapor generally occur in crowded parts of the spectrum, so that the widening attributed to them may really be due to the presence of faint solar lines in their immediate neighborhood. Again, there is a haziness about the widening of such lines which is unlike the clear-cut widening of metallic lines. Hence the appearance of widening may be purely subjective and so fictitious, and not really due to the spot. The question is one that needs to be elucidated by further research.

The predominance of vanadium and titanium in Sun-spots is important in view of Professor Fowler's recent identification of the flutings in Secchi's third type of stars as due to titanium or to a compound of the metal,[2] and Sir Norman Lockyer's matching of the intensified lines in the spectrum of *Arcturus* with lines of these same elements.[3] Professor Hale has also shown that many of the lines in stars of the fourth type are coincident with lines observed in the spectra of Sun-spots by Mr. Maunder and myself.[4]

Stonyhurst College Observatory,
July, 1904.

[1] *Monthly Notices, R. A. S.*, **47**, 19, 1886.

[2] *Proc. R. S.*, **73**, 219, 1904. [3] *Ibid.*, **74**, 53, 1904.

[4] *Publications of the Yerkes Observatory of the University of Chicago*, **2**, "The Spectra of Stars of Secchi's Fourth Type," by Messrs. Hale, Ellerman and Parkhurst.

ON THE PRESENCE OF YTTRIUM AND YTTERBIUM IN FLUOR-SPAR.

By W. J. HUMPHREYS.

SOME years ago fluor-spar was found at Amelia Court House, Virginia, so sensitive to thermal effects, that on simply being kept in the hands for a few minutes it becomes distinctly luminous. This exceptional sensitiveness naturally rendered it an object of some interest, and in the spring of 1903 I got several samples from the supply of it at the University of Virginia with the object of examining it spectroscopically. The phosphorescent light of the green specimens gave only a broad green band, at least in the visible region, while the pinkish ones gave, besides the green, a somewhat fainter orange band. The light, however, was too feeble to justify an attempt, through wide dispersion, at possible resolution and wavelength determination, and this part of the investigation was therefore abandoned; but with the hope that either the spark spectra of any included volatile substances or the arc spectrum of the solid itself might furnish more interesting results.

In the meantime Dr. Charles Baskerville, then of the University of North Carolina, sent me several samples of powdered fluor-spar with the request that I examine them for the possible presence of thorium, an element, or complex, to which he has devoted a great deal of valuable work. The origin of these samples was at first a secret, but some months later I was informed that they too were from Amelia Court House. Baskerville was then working on this particular fluor-spar with Kunz of New York city, and doubtless in due time they will have much of interest to report. No thorium was found, but arc spectra of all the samples showed the presence of quite appreciable amounts of yttrium and ytterbium. On examining my own samples of fluor-spar from Amelia Court House the same results were obtained, and as neither of these elements, so far as I could learn, had ever previously been detected in this rather common and widely distributed mineral, the examination for them was extended to samples from other localities, with the results shown in Table I.

TABLE I.

List of Fluor-Spars Examined.

Where from	Furnished by	Amount Yttrium	Amount Ytterbium
AFRICA: *Dutch S. W.*—			
Hanneit am Bockberge	K.	Appreciable	
AMERICA: *Canada*—			
Thunder Bay, Manitoba	F.	Fair	Trace
AMERICA: *Mexico*—			
Near Guanajuato	F.	Trace	
AMERICA, U. S: *Arizona*—			
Yuma Co.	F.	Appreciable	
California—			
Los Angeles	P. L.	Small	
White Pine (Colo. ?)	P. L.	Small	
Colorado—			
Eureka	F.	Fair	Trace
Pike's Peak	F.	Small	
Connecticut—			
Long Hill	F.	Small	Trace
Plymouth	E. B. M.	Small	
Trumbull	A. H. P.	Small	
Illinois—			
Hardin Co.	A. J. M.	Trace	
Rosiclare	J. W. M.	Trace	
Shawneetown	A. C. G.	Trace	
Kentucky—			
Ashbridge Spar, Crittenden Co.	B. and N.	Trace	
Columbia Mine,[1] Crittenden Co.	B. and N.	Small	
Corn Spar, Crittenden Co.	B. and N.		
Harrodsburg, Mercer Co.	C.	Trace	
Hodge Mine, Crittenden Co.	B. and N.		
Klondike Mine, Crittenden Co.	B. and N.		
Memphis Mine, Crittenden Co.	B. and N.		
Panther Hollow Mine, Crittenden Co.	B. and N.		
Tabb Mine, Crittenden Co.	B. and N.		
Maine—			
Wilson	N. E. G.	Trace	
Maryland—			
Near Cumberland	E. B. M.		
Missouri—			
Barry Co. (?)	W. G. B.	Trace	
St. Louis	J. W. M.	Trace	

[1] Lead and zinc mine with small amount of fluor-spar.

TABLE I—*Continued.*

List of Fluor-Spars Examined.

Where from	Furnished by	Amount Yttrium	Amount Ytterbium
New Hampshire—			
Westmoreland	J. W. M.	Small	
White Mountains (colorless)	N. E. G.	Fair	
White Mountains (green)	N. E. G.	Fair	Trace
New Jersey—			
Franklin	E. B. M.	Small	
Hamburg	A. J. M.	Small	
New Mexico—			
Socoro Co.[1]	E.	Fair	
New York—			
Gouverneur	A. J. M.	Small	
Lockport	F.		
McComb	C. B.	Fair	
Mineville	A. C. G.	Fair	Small
Muscolognc Lake	J. W. M.	Small	
"New York"	E. B. M.	Trace	
Rochester	E. B. M.		
Tilly Foster Mine, Brewster	F.		
North Carolina—			
Davidson Co.	C. B.	Small	
Ohio—			
Carlysle	E. B. M.	Trace	
Pennsylvania—			
Chester Co.	F.	Small	
Monroe Co.	F.		
Tennessee—			
Davidson Co.	W. L. D.	Small	
Smith Co.	W. L. D.	Small	
Watertown	W. L. D.	Trace	
Texas—			
Llano Co.	H. W. H.	Small	
Llano Co.[2]	H. W. H.	Large	Large
Utah—			
Woodside Mine, Park City	J. F. K.	Small	
Virginia—			
Amelia Court House	J. W. M.	Large	Small
Amelia Court House (green)	C. B.	Fair	Large
Amelia Court House (purple)	C. B.	Fair	Fair
Amelia Court House (white)	C. B.	Large	Fair
Fabers Mills	F. P. D.	Small	

[1] Contains a large amount of barium.

[2] From region of gadolinite and other rare minerals.

TABLE I—*Continued.*

List of Fluor-Spars Examined.

Where from	Furnished by	Amount Yttrium	Amount Ytterbium
AMERICA, SOUTH: *Bolivia*—			
Corocoro	J. F. K.	Large	Small
ASIA: *Siberia*—			
Nertchinsk	A. J. M.	Small	
AUSTRALIA: *N. S. Wales*—			
Block 14 Mine, Broken Hill	K.	Small	
EUROPE: *Austria*—			
Altenmarkt, Salzburg	W. L. D.	Small	
Bleiberg, Carinthia	K.	Trace	
Graupen, Bohemia	W. L. D.	Small	
Moldova, Banat, Hungary	K.	Small	
Rabenstein, Tyrol	K.	Small	
Sarnthal, Tyrol	K.	Fair	Small
Schlackinwald, Bohemia	E. B. M.	Trace	
Schönfeld, Bohemia	K.	Small	
Sodenthal, by Bozen, Tyrol	K.	Small	
Weipert, Bohemia	K.	Small	
Zinwald, Bohemia	E.	Small	
England—			
Allen Heads, Northumberland	C. B.	Fair	
Alston Moor, Cumberland	J. W. M.	Fair	Small
Beer Alston, Devonshire	W. L. D.	Small	
Cornwall	F.	Small	
Derbyshire	F.	Small	
Devonshire	K.	Small	
Northumberland	C. B.	Fair	
Weardale, Durham	J. W. M.	Fair	Small
France—			
Auvergne	W. L. D.	Small	
Gabas, Pyrenees	K.	Small	
Germany—			
Altenberg, Saxony	A. J. M.	Appreciable	Trace ?
Andreasberg, Harz	W. L. D.	Appreciable	
Bösenbrunn, Saxony	K.	Fair	
Braunsdorf, Saxony	K.	Fair	
Celsnitz, Saxony	A. J. M.	Trace	
Ehrenfriedersdorf, Saxony	E.	Small	
Freiberg, Saxony	W. L. D.	Appreciable	
Furstenberg, Saxony	W. L. D.	Small	
Gersdorf, Saxony	K.	Small	
Grosschirma, Saxony	K.	Fair	Trace
Grube Moritz, by Sewen, Upper Alsace	K.	Small	
Haardt, by Kreuznach, Rhine	K.	Fair	Trace
Hausbaden, Baden	K.	Small	
Kiesberg, Silesia	K.	Trace	
Krumschlactthal, Stolberg	W. L. D.	Fair	

TABLE I—*Continued.*

List of Fluor-Spars Examined.

Where from	Furnished by	Amount Yttrium	Amount Ytterbium
Marienberg, Saxony	F.	Small	
Munsterthal, Baden	A. J. M.	Fair	Trace
Neudorf, Harz	E. B. M.	Fair	
Niederpöbel, Saxony	K.	Fair	Trace ?
Rappoltsweiler, Alsace	K.	Small	
Ratisbon, Bavaria	K.	Trace	
Schleusingen	K.	Fair	Small
Schmalkalden, Kurhessen	W. L. D.	Small	
Schwarzenberg, Saxony	K.	Small	
Stolberg, Harz	J. W. M.	Appreciable	
Strassberg, Harz	K.	Small	
Striegau, Silesia	K.	Small	
Welsendorf, Bavaria	W. L. D.	Small	
Woelsenberg, Bavaria	A. J. M.	Trace ?	
Worlberg, Harz	J. W. M.	Appreciable	Trace ?
Italy—			
Baveno	F.	Small	
Gerfalco, Tuscany	K.	Small	
Vesuvius	K.	Small	
Norway—			
Kongsberg	K.	Small	
Kragerö	K.	Trace	
Switzerland—			
Dissentis	J. W. M.	Small	
Fiesch	K.	Fair	Trace
Oltschen Alps	K.	Small	
St. Gothard	E.	Fair	Appreciable

EXPLANATION OF TABLE I.

In the columns marked "Amount," the order of increase is trace, small, appreciable, fair, large. The first is used when the lines of the element in question are clearly present but faint, while the last is reserved for those samples that yield very heavy lines.

In the column designated "Furnished by,"

C. B. means Dr. Charles Baskerville, University of North Carolina.

B. and N. means Blue and Nunn, Marion, Ky.

W. G. B. means Professor W. G. Brown, University of Missouri.

C. means Chinn Mineral Co., Harrodsburg, Ky.

W. L. D. means Professor W. L. Dudley, Vanderbilt University, Tenn.

F. P. D. means Professor F. P. Dunnington, University of Virginia.

E. means George L. English Mineral Co., New York city.

F. means Foote Mineral Co., Philadelphia, Pa.

N. E. G. means Dr. N. E. Gilbert, Dartmouth College, New Hampshire.

A. C. G. means Professor A. C. Gill, Cornell University, New York.

H. W. H. means Dr. H. W. Harper, University of Texas.

J. F. K.[1] means Professor J. F. Kemp, Columbia University, New York city.
K. means Dr. F. Krantz, Bonn, Germany.
P. L. means Dr. P. Lewis, University of California.
J. W. M. means Dr. J. W. Mallet, University of Virginia.
E. B. M. means Dr. E. B. Mathews, Johns Hopkins University, Maryland.
A. J. M.[1] means Professor A. J. Moses, Columbia University, New York city.
A. H. P. means Professor A. H. Phillips, Princeton University, New Jersey.

The above table by no means exhausts the fluor-spar localities, but it is sufficient to show the wide distribution of yttrium and ytterbium. The failure to detect these elements in certain fluor-spars does not at all prove their absence, but only that if present they are there in vanishingly small amounts. In this connection the fluor-spars of Crittenden county, Ky., are of interest. There are several large fluor-spar mines in this section where the mineral is obtained commercially, and also a lead and zinc mine that furnishes a small quantity of fluor-spar. This latter contains a distinct amount of yttrium while samples taken from the large mines do not show it. In the one case it is relatively concentrated, while in the other it probably is also present, since found in that region, but too diluted by the great mass of fluor-spar to be detected by ordinary means.

Along with the fluor-spar at Amelia Court House occur several other minerals, and of these allanite, beryl, columbite, feldspar, garnet, microlite, and monazite were examined. Yttrium was found in all of them and, as with the fluor-spars, where the yttrium was in quantity ytterbium was also detected. This region and a part at least of Llano county, Texas, seem to be especially rich in these elements, and while their presence in the fluor-spars of these places might therefore not be surprising, it is certainly worth noting that yttrium and probably, to a less extent as a rule, ytterbium as well, occurs in practically all fluor-spars; and further that these alone so occur to any appreciable extent. None of the other rare elements, such as cerium, lanthanum, the didymiums, etc., was detected in even those fluor-spars from the regions of these elements and rich in yttrium and ytterbium.

The three samples richest in yttrium and ytterbium, those from Amelia Court House, Va., Llano county, Texas, and Corocoro, Bolivia, proved to be exceptionally sensitive to thermal effects.

[1] Through the kindness of Dr. S. A. Mitchell, Columbia University, New York.

This, while it may be only a coincidence, seems to fit with the idea that the phosphorescence is conditioned by some sort of molecular instability.

It may also be worth noting that there seems to be no sort of connection between the amounts of yttrium and ytterbium present and the color or the transparency of the fluor-spar.

A few other minerals, such as marble, borocalcite, cryolite, and mica, were examined to see whether yttrium follows either calcium or fluorine compounds in general, but it was not with certainty detected in any of them.

Possibly these facts may be of interest in mineralogy and chemical geology, but I leave any discussion of this to those more directly concerned with these subjects. Be that as it may, the wide distribution of yttrium and ytterbium is certainly of some value to astrophysics, because it quite prepares one to look for their lines in the solar spectrum. Indeed Rowland's tables assign a considerable number of lines to yttrium, but several other yttrium lines and a few of ytterbium I think may safely be added.

These are given in Table II, and while the agreement in wave-length between Exner and Haschek's tables of spark spectra, which are used for this correction, and Rowland's solar spectrum is not exact, it probably is as close as the difference in sources and methods of observation and measurement could lead one to expect.

The chief though not the only yttrium lines used in this search are λ 4309.81 and λ 4375.12. The latter is the strongest line in the arc spectrum of yttrium and is well removed from other spectrum lines, including bands, except cobalt λ 4375.11, manganese λ 4375.10, and samarium λ 4375.15, all of which are faint. None of even the strongest cobalt and samarium lines was found on any of the plates, and while manganese was always detected, owing to its presence in the carbons, and probably too as a trace in some of the fluor-spars, only its strongest lines appeared. Several plates were taken with bare poles, and in no case could λ 4375.10 be found. Consequently under the circumstances a line at this place was tolerably conclusive evidence of the presence of yttrium; and besides, whenever it was at all heavy, other yttrium lines, in their proper relative intensities, were always present.

Similarly in the case of ytterbium the principal lines used are λ 3694.37 and λ 3988.16. The latter is tolerably though not wholly free from band disturbance, is much the strongest of all ytterbium lines, and is well isolated, except for uranium λ 3988.16 and tungsten λ 3988.18, neither of which could have appeared on my plates, since both are faint and not even the strongest lines of these elements were ever present.

The above wave-lengths are those given by Exner and Haschek for arc spectra.

In all cases the first spectrum as produced by a Rowland concave grating of 21.5 feet (6.5 meters) focal length was used, and an effort was made to have the exposures the same, though owing to great irregularities in the arc this equality must be only very roughly approximate.

TABLE II.

Suggested Corrections to Rowland's Table.

Exner and Haschek's Spark Spectra			Rowland's Solar Spectrum		
λ	Substance	Intensity	λ	Substance	Intensity
3289.52	*Yb*	100	3289.498	*Eb* ?	4
3478.99	*Yb*	20	3479.053		000
3694.35	*Yb*	200	3694.344	*Eb* ?	3
3747.70	*Y*	8	3747.694		1
3818.49	*Y*	10	3818.487		1
3833.00	*Y*	20	3833.026	—, *Ni*	3*n*
3988.16	*Yb*	15	3988.114	*Eb* ?	0
4143.03	*Y*	7	4143.000		00
4177.65	*Y*	50	4177.698		3
4235.94	*Y*	10	4235.994		0
4309.81	*Y*	20	4309.792		1
4375.11	*Y*	100	4375.103	*V*, *Mn*	2
4398.21	*Y*	15	4398.178		1

In closing I wish to say that the fluor-spars, except of course those furnished by English, Foote, and Krantz, were sent to me without charge, and I desire here to thank all those whose combined kindness made this examination possible.

University of Virginia,
July 1904.

ON THE CONDITIONS WHICH GOVERN THE APPEARANCE OF SPARK LINES IN ARC SPECTRA.[1]

By Henry Crew.

The spark lines of any element are defined as those which appear in its spark spectrum without appearing in its arc spectrum, or, if found in the arc, appear much weaker than in the spark.

If we adopt the unitary view of the electric discharge, as set forth by J. J. Thomson,[2] J. Stark,[3] and others, we may perhaps think of the spark in air as the general case of which the arc is a highly special case. But very important differences have been observed between the spectrum of this momentary arc, with its apparently small current between relatively cold electrodes, and that of the ordinary arc produced by a large uninterrupted current between relatively hot electrodes. The most fundamental of these differences are perhaps the following:

1. The spark lines are in general less sharp than the arc lines.
2. The spark lines are stronger and more numerous in the ultra-violet.
3. The minimum potential difference required to produce a spark (except across distances comparable with a wave-length of light, such as those studied by Earhart[4]) is about 350 volts, roughly ten times that required to maintain an arc.
4. Spark lines are in general accompanied by air lines.

To determine as completely as possible the essential physical conditions, in the source, which produce the one or the other of these two spectra may be reckoned one of the important problems of present-day spectroscopy.

Adopting the electron theory, one naturally expects to find most of the arc lines in the spectrum of the spark; but the discovery of the inverse phenomenon by Liveing and Dewar[5]—the appearance of

[1] A paper read before the American Physical Society, September 16, 1904.

[2] *Conduction of Electricity through Gases*, chaps. 13 and 14, 1903.

[3] "Entstehung der electrischen Gasspectren," *Ann. der Phys.*, **14**, 506, 1904.

[4] *Phil. Mag.*, **1**, 147, 1901. [5] *Proc. R. S.*, **44**, 241, 1888.

purely spark lines in arc spectra—is somewhat surprising.

The first line of this type to be observed was λ 4481 in the arc between *Mg* electrodes. Since then many others have been noted, among them being the *Zn* line at λ 5894, between the two D's, the red *Cd* line at λ 6438, and the *Al* line at λ 2816.

In the oscillatory spark, Schenck[1] found this type of line "due entirely to the oscillations, and the arc lines due partly to the oscillations and partly to something else which retains luminosity after the oscillations cease." What corresponding distinction can be made in the case of the "dead-beat" discharge is an interesting question. In any event, the presence of spark lines in the arc would seem to prove that the oscillatory discharge is not a *conditio sine qua non* for their appearance.

In the case of the arc, it has been found by the writer[2] that an atmosphere of hydrogen greatly enhances the intensities of the spark lines relatively to those of the arc. Porter[3] has shown that atmospheres of ammonia and oxygen produce a similar effect. Hartmann and Eberhard[4] have shown that the submersion of the arc under water serves to intensify the spark lines, owing probably to electrolytic hydrogen set free in the neighborhood of the electrodes. Hartmann[5] has discovered that in the arc with stationary metallic electrodes some spark lines, *e. g.*, in *Mg*, *Pb*, and *Bi*, increase in relative intensity as the current dim'nishes. He also found that the appearance of these lines is not due to the amount of *impressed* voltage, since a current of 4 amperes under 120 volts yields *Mg* λ 4481 only faintly, while the same current under 20 volts gives this line thirty times as strong. But I am inclined to give these results an interpretation different from that of Hartmann. For voltage is not the only variable here introduced; the two arcs differ in another very essential respect, viz., with a pressure of 20 volts the electrodes are comparatively cool, while with 120 volts the electrodes are enormously hot—white-hot indeed, since they yield a continuous spectrum. With electrodes at these high temperatures it is well known that *Mg* λ 4481 does not

[1] ASTROPHYSICAL JOURNAL, **14**, 126, 1901.

[2] ASTROPHYSICAL JOURNAL, **12**, 167, 1900. [3] *Ibid.*, **15**, 274, 1902.

[4] *Sitzber. Berl. Akad.*, 22 Jan. 1903; ASTROPHYSICAL JOURNAL, **17**, 229, 1903.

[5] *Sitzber. Berl. Akad.*, 26 Feb. 1903; ASTROPHYSICAL JOURNAL **17**, 270, 1903.

appear at all, or, at most, appears only in a region close up to the electrodes. May it not then be wiser to interpret this experiment as proving that the effect of increased voltage is here unable to overcome the effect of hot electrodes? One is confirmed in this interpretation by an experiment of Basquin's[1] in which he shows that voltage can, if it be high enough, and if it change with sufficient rapidity, produce spark lines even in a powerful carbon-metal arc where the electrodes are close together and white-hot.

The following experiments, generously made possible by the Car-

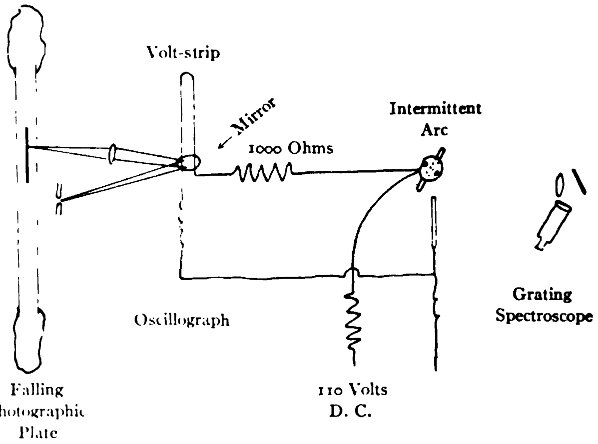


FIG. 1.—Plan of Apparatus.

negie Institution, were undertaken to determine, as nearly as possible, the electrical conditions which accompany the appearance of the spark line at λ 4481, in the *Mg* arc. For this purpose the arc was examined simultaneously with a Rowland plane-grating spectroscope and with a Duddell high-frequency oscillograph arranged as indicated in Fig. 1.

The collimating lens (5 feet focal length) was used without a slit in order to make each line a monochromatic image of the arc. The oscillograph was provided with a falling photographic plate; its frequency was ten thousand vibrations per second, and its oil damping practically perfect The scale employed on the time-axis of the photographs was uniformly 1/4000 second to the millimeter. The

[1] ASTROPHYSICAL JOURNAL, 14, 14, 1901.

volt-strip of the oscillograph was shunted across the electrodes of the arc. The voltage employed was 110 direct.

Case I. Circuit with negligible inductance.—Such a circuit is obtained by joining the arc in series with the generator and with the

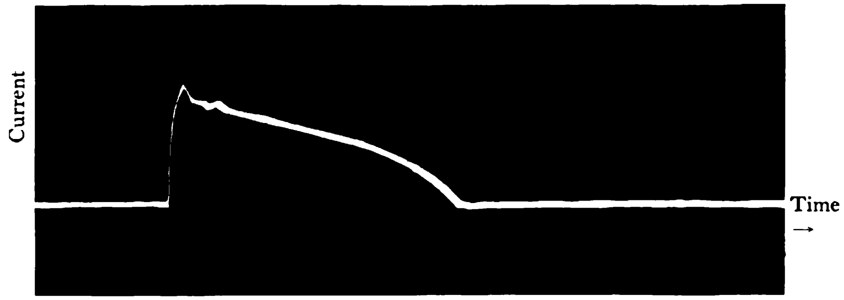


FIG. 2.—Current-curve of magnesium arc.

proper non-inductive resistance; the only inductance in the circuit is then the negligible amount located in the armature. The line at λ 4481 is found only in the immediate vicinity of the electrodes. The

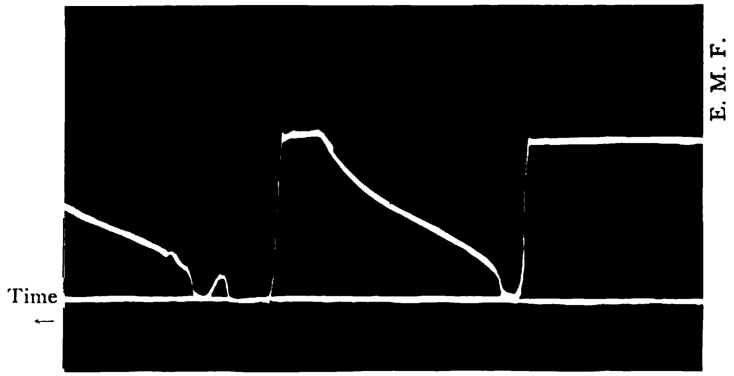


FIG. 3.—E. M. F. Curve; iron arc; no inductance.

typical current-curve[1] in this case is shown in Fig. 2; the typical E. M. F. curve is reproduced in Fig. 3. Scarcely a trace of extra E. M. F. is found when the arc is broken.

[1] It was hoped that these curves could be reproduced without any retouching: but at the time they were photographed the mirrors of the oscillograph had deteriorated to such an extent that retouching became a necessity. However, all the essentials and practically every detail of the curves remain unchanged.

Case II. Circuit with large inductance.—A large electromagnet, whose inductance was 0.03 henrys, was placed in series with the arc. The appearance of λ 4481 was practically unchanged from the preceding case, the region in which the line appears being limited to the immediate vicinity of the electrodes. But the E. M. F. curve is much less abrupt in its changes than in the circuit without inductance. Fig. 4 represents a typical E. M. F. curve in this case.

It might be expected that under these circumstances—namely, rather large inductance, open magnetic circuit, single electric circuit—

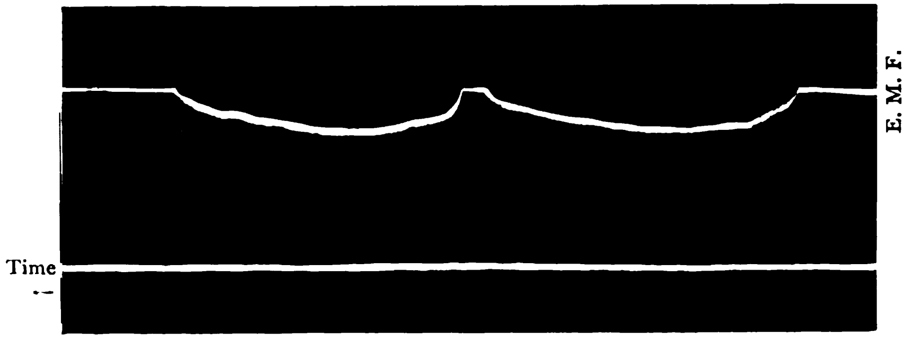


FIG. 4.— E. M. F. Curve; iron arc; large inductance.

there would occur at break a sudden fall of current, accompanied by a large extra E. M. F. The painful physiological effect of shunting such a viciously intermittent arc with one's body might at least warrant this expectation. The oscillograph shows, however, that the break with high inductance is not rapid; the current appears to be "let down easy" by the metallic vapor in the arc. It may be that with large inductance, the extra current feeds the arc with metallic vapor until the very end of the break, which occurs with a suddenness such that it produces an *exceedingly brief* extra E. M. F., affecting the nerves, but not affecting the volt-strip of the oscillograph. In any event, the important fact is that no extra-voltage is observable on the instrument.

Case III. Arc broken by air-blast.—In the ordinary intermittent arc just described, λ 4481 is found only in the immediate neighborhood of the electrodes. Now and then the line may be observed to

shoot clear across from one electrode to the other; but this does not happen under normal or steady conditions. Accordingly, the next step was to obtain an arc which would yield *Mg* λ 4481 all the time and all the way across from one electrode to the other. This was done by ntroducing, behind the arc, an air-blast directed at right angles to the arc and along the axis of the collimator, and at the same time removing from the circuit, as nearly as possible, all inductance.

The effect of this quickened break is to bring out λ 4481 very strongly from one electrode to the other, and simultaneously to bring

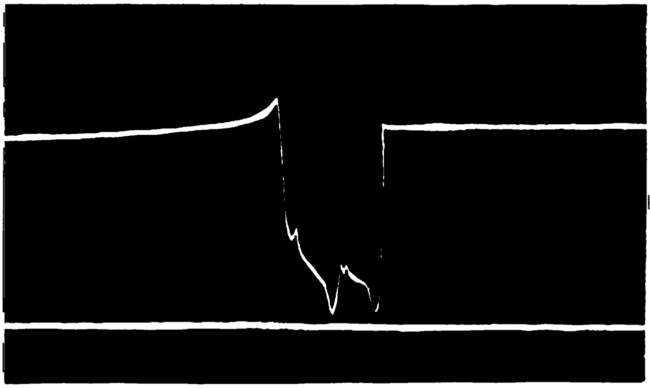


FIG. 5. E. M. F. Curve of *Mg* arc interrupted by air-blast.

out a distinct extra E. M. F. in the voltage-curve of the oscillograph, as shown in Fig. 5.

The photograph shows the amount of this extra E. M. F. to be something like 10 per cent. added to he impressed E. M. F The same type of curve is given by iron and by other metallic arcs, one of which is shown in Fig. 6.

In order to show that this peculiar feature of these E. M. F. curves does not hinge upon any peculiarity of the oscillograph, I photographed the curve with every condition, including the air-blast, the same as before, except that an inductance of 0.03 henrys, having negligible resistance, was introduced. The effect was to destroy the last trace of any extra E. M. F., which could hardly have happened if the curve in Fig. 6 had in any way resulted from lack of "dead-beatness" in

the vibrator of the oscillograph. The simultaneous effect in the spectroscope is to practically obliterate λ 4481.

Case IV. Arc in atmosphere of coal gas.—The next case examined was that in which spark lines are introduced into the arc by means of atmospheres of hydrogen, coal gas, ammonia, etc. The arc was here inclosed in coal gas and examined as before with the spectroscope and the oscillograph. The same extra E. M. F. and the same relative intensification of λ 4481 were found. Similar phenomena were observed in an atmosphere of oxygen.

When the arc is operated in transformer oil, the same extra E. M.

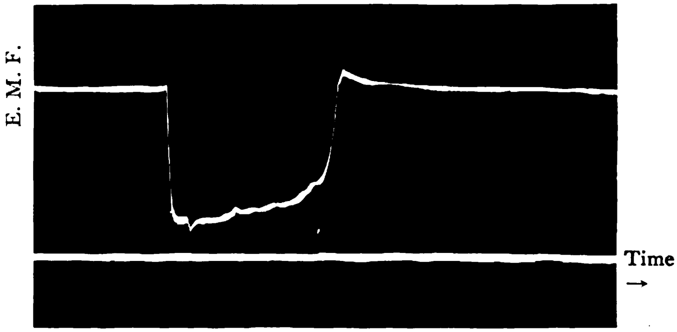


FIG. 6.—E. M. F. Curve of iron arc, interrupted by air-blast.

F. is observed; but, under the circumstances of this experiment, the absorption of light in the oil was sufficient to blot out almost the entire spectrum of magnesium, so that nothing can be said about the intensity of λ 4481.

We have here probably the explanation of the fact that spark lines are either introduced or intensified by the above-mentioned atmospheres; namely, *the hydrogen, for example, makes a more rapid break, and this in turn introduces an extra electromotive force which in some way, as yet unknown, is responsible for the radiation of the spark line.*

It is not to be imagined that in any of these cases the small extra E. M. F. observed, say 10 volts, is entirely responsible for the appearance of the spark lines. Much more probable is it that this rise of 10 volts is merely the indication which the oscillograph gives of a

vastly higher E. M. F. lasting for a time too short to record itself on this instrument.

It is evident from the form of these voltage-curves, considered in connection with a simultaneous current-curve, as shown in Fig. 7, that the E. M. F. in the arc diminishes, *ceteris paribus*, as the current rises.

In a constant voltage circuit small currents will therefore carry with them a higher voltage between the electrodes, cooler electrodes, and in all probability an enormously quicker break (and hence higher extra E. M. F.) than the larger currents.

It seems, therefore, not unlikely that *in the case of the small cur-*

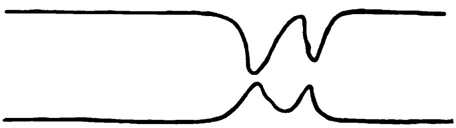


FIG. 7.—Illustrating fall of voltage with rise of current in arc circuits.

rents studied by Hartmann, as well as in the case of the arc interrupted by an air-blast; in the case of the arc in hydrogen, etc., and in the case of the ordinary spark, the one condition essential for the production of Mg λ 4481 is a voltage relatively high when compared with that necessary to produce arc lines alone.

Just what extra E. M. F. ought to be expected at the break of an ordinary metallic arc it is difficult to say, for the reason that the differential equation which describes the electrical behavior of such a circuit

$$\frac{d(Li)}{dt}+Ri=E$$

is not immediately integrable. E is here the impressed E. M. F., L the inductance, and i the instantaneous value of the current. The difficulty is that R, which is the resistance of the entire circuit *including the arc-gap*, is not known as a function of the time, t; and hence the extra E. M. F. cannot be computed.

But Arons[1] has assumed a functional relation between R and t, which, it seems to me, describes fairly well the circumstances of the intermittent arc when operated without inductance. He imagines

[1] *Wied. Ann.*, **63**, 177, 1897.

the resistance of the circuit to increase uniformly with the time from its initial value, R, to infinity, the whole variation occupying a very brief interval of time, say of the order of 1/1000 second, extending from $t=0$ to $t=\tau$.

This assumption, which would seem to be justified by the typical current-curve shown in Fig. 2, makes the equation easily integrable, and allows us by again differentiating the integral value of i to get $\frac{d(Li)}{dt}$ in terms of known quantities, that is, in terms of quantities not involving i, namely,

$$\text{Extra E. M. F.} = \frac{d(Li)}{dt} = E\frac{\theta}{\theta-\tau}\left\{\left(\frac{\tau}{\tau-t}\right)^{\frac{\theta-\tau}{\theta}} - 1\right\}$$

where θ is the time-constant of the circuit, τ the entire duration of the break, and t the instant at which the extra E. M. F. is computed.

Applying Arons' solution to Case I above, where an electrode 2 inches long is rotating about its center with a speed of 1200 revolutions per minute, and where the arc appears to break about the time it reaches $\frac{1}{4}$ inch in length, we find $\tau=0.002$ second approximately. The initial resistance is here 10 ohms, and the inductance approximately $\frac{1}{3}$ of a milhenry; so that the time-constant is 0.00003 second.

For cases of this kind, in which $\tau > \theta$, Arons' equation becomes

$$\frac{d(Li)}{dt} = E\frac{\theta}{\tau-\theta}.$$

Accordingly, we have, for a 110-volt circuit, an extra E. M. F. of $110\left(\frac{3}{200-3}\right) = 1.6$ volts, extending over all except the very first instant of the break. Such a small increase of voltage—approximately 1 per cent.—could scarcely be detected on this oscillograph. It is not surprising, therefore, that no extra E. M. F. was found on any of the voltage-curves obtained with a natural break.

In Case II, where a large inductance—30 milhenrys—is introduced, the current-curve during break is no longer a straight line, but is convex outwards, so that Arons' assumption of a uniformly increasing resistance can hardly fit the case; for here the extra current is very large and appears to fill the arc with metallic vapor and to delay the high resistance until the very end of the break.

It is not surprising, therefore, that we do not here find the large extra voltages predicted by Arons' equation, ranging from 11.6 volts, very near the beginning, to 1940 volts, very near the end of the break.

In Cases III and IV, however, where inductance has been removed and the break artificially quickened, Arons' assumption appears to be at once the simplest and most rational. If we suppose that the air-blast has reduced the duration of the break to one-fifth or one-tenth of its former value, then Arons' equation predicts an extra E. M. F. of from 9 to 19 volts, in addition to the impressed E. M. F., which agrees very well with that actually observed in the oscillograph.

Stark (*loc. cit.*, p. 519) makes a rather clear distinction between the arc and spark, based upon two antecedent definitions. He first defines a purely thermal temperature in the ordinary manner, namely, makes it proportional, for any one gas, to the mean square of the molecular speeds. He next defines an "electrical temperature" as one proportional to the mean square of the ionic speeds. It is evident that in the case of a strong electric field, such as that between the electrodes of a spark, the ionic velocities will be distributed in a manner very different from that prescribed by the Maxwell-Boltzmann law, and that the "electrical temperatures" may differ very widely from the "thermal."

The experiments described above seem to point to Stark's distinction between the arc and spark as a valid one. His distinction is roughly as follows: The arc is a region of comparatively high thermal and low electrical temperature, while the spark is a seat of comparatively low thermal and high electrical temperature May it not be that the lack of this distinction has caused much fruitless discussion of the perennial question concerning the temperatures of these two sources?

Another step in this direction may lead us to what is rather pure speculation. Yet those who have studied electrical vibrations and those who have studied the Geissler tube have given us evidence for thinking that the source of radiation in such a discharge, and in electrical discharges in general, lies in the accelerated negative ion.

In the case of the vacuum tube, where the electric discharge is presented if not in its simplest form, at least in a form most easily studied, it has been amply proved, by Graham, Wood, and others,

that *ionization*, *luminosity*, *temperature*, and *electric force* rise and fall together. Just how this may happen has been beautifully explained by Thomson in his *Conduction of Electricity through Gases*, Chapter XVI.

The evidence cited above for thinking that spark lines are always accompanied by steeper potential-gradients would seem therefore to be quite in harmony with the electron theory, in which radiation is proportional to the square of the acceleration[1] of the electron, and the acceleration in turn proportional to the potential-gradient.

In the case of those stars which exhibit spark lines in their spectra, it is difficult to imagine just what similarity of physical condition there is between the star and the region about the electrodes, especially since we might expect stellar temperatures to be what Stark calls "purely thermal," while in the spark "electrical temperatures" would be the dominant factor. Is it not possible that, under the extraordinary temperatures which may exist in stellar bodies, the ionic speeds, while differing radically in distribution, may be quite equal in amount to those which occur in the most energetic spark? If so, the physical difference between the two sources would be slight, for it would be reduced largely to *a difference of distribution of ionic velocities*. The chief results of this experiment are then the following:

1. A high E. M. F., rapidly changing, is a probable *conditio sine qua non* for the appearance of spark lines in arc spectra.

2. The effect of hydrogen and other atmospheres in introducing spark lines is explained by the fact that these atmospheres produce a more rapid break.

Northwestern University,
Evanston, Ill.
September 1, 1904.

[1] Larmor, *Ether and Matter*, p. 227.

THE LOSS OF LIGHT BY DIFFRACTION AT A NARROW SLIT.[1]

By J. H. Moore.

It is well known that all high-dispersion slit spectrographs are very wasteful of light. When they are used in conjunction with a refractor, the combined instrument delivers to the photographic plate probably less than 10 per cent. of the light incident upon the objective of the telescope. In the design of astronomical spectrographs, where the source of light is in general faint, it becomes then of fundamental importance to arrange the parts of the instrument so that it shall have the greatest possible efficiency, consistent with the purity of spectrum, dispersion, and resolving power required by the particular problem in hand.

If a certain resolving power and dispersion be given, then in a prism-spectrograph a definite and unavoidable loss of light occurs by absorption and reflection at the objective, the lenses and prisms. To secure the required purity of spectrum it is necessary to use a very narrow slit, introducing an additional loss of light which may be looked upon as due to two causes: first, the diminished area of the image source and, second, diffraction at a narrow slit. Both of these losses depend upon the linear width of slit, while the purity of the spectrum depends only upon the angular slit-aperture as seen from the center of the collimator lens. It is therefore possible, by using a collimator of sufficient focal length, to preserve the purity of spectrum, and at the same time utilize the light which would otherwise be lost at a narrow slit.

The importance of using a collimator of great focal length so as to increase the linear aperture of the slit was first pointed out by Professor Campbell,[2] who, in the design of the Mills spectrograph has employed a collimator length limited only by the size of the prisms which it is advisable to use. Abbot,[3] in his researches with

[1] Also to appear as a *Bulletin* of the Lick Observatory.

[2] "The Mills Spectrograph of the Lick Observatory," Astrophysical Journal, **8**, 123–156, 1898.

[3] *Annals of the Astrophysical Observatory of the Smithsonian Institution*, **1**, 1900.

a bolometer, has shown that the loss of light at a narrow slit due to diffraction alone is quite an important factor, amounting to about 50 per cent. with the particular apparatus, slit-widths (0.10 mm), and region of spectrum (1.8 μ) used in his investigation. With the region of spectrum (about 0.45 μ) employed in line-of-sight work the loss by diffraction does not amount to so much as this with the linear aperture of slit which it is possible to use; but it is desirable to know just what this loss is for different slit-widths. At the suggestion of Director Campbell, the present investigation was undertaken on account of its important bearing upon the design of astronomical spectrographs.

As we are interested here only in photographic effects, it was decided to use photographic methods in the investigation.

Experience has shown that it is possible to judge of relative intensities by the density of the corresponding photographic image only within certain limits, but a method which reduces the comparison of intensities to one of exposure-times giving the same density of negative seems to be quite accurate, and assumes nothing with regard to the action of a photographic plate which is not in accord with experiment. The following method was found to be very sensitive to changes of intensity, and was employed in the present work. The spectrum of a constant source of light was photographed with different apertures of slit. One slit-width with a given exposure-time was selected as standard. The intensities due to other widths of slit were compared by giving with each width of slit the exposure required to produce the same density as that from the standard slit. The ratio of the time of exposure for any given slit-aperture to that for the standard slit is taken as being inversely proportional to the photographic intensities in the two cases. Any slight error arising from such an assumption is irrelevant in this problem.

The instruments used in the present work were the thirty-six-inch refractor and the new Mills spectrograph, the most of whose constants are the same as for the former instrument, described by Professor Campbell. This spectrograph is provided with a bilateral slit, the pitch of whose screw is 0.01 inch. The milled head of the screw is divided to twenty parts, so that the slit-width corresponding to one division is 0.001 inch. The focal length of the collimator is

722.4 mm, and its aperture is 38 mm. A special sliding plate-holder was designed and used which permits about twenty exposures to be made upon the same plate. The time of exposure was determined from a relay sounder operated by one of the standard clocks. Cramer "Crown" plates of the same emulsion were used in all of the experiments.

To obtain a constant source of light the telescope was pointed to the region of sky 90° north declination and hour-angle differing six hours from that of the Sun. The driving-clock was run in order to eliminate the effect of the variation in the plane of polarization of the sky light, with respect to the plane of the instrument, due to the change in the hour-angle of the Sun. This source was found to remain fairly constant on a cloudless day when the hour-angle of the Sun was not greater than one hour.

A slit-width was selected as standard which would give a purity of spectrum comparable with that from apertures throughout the range over which the experiments extended. A standard width of 0.002 inch (0.05 mm) was found to be satisfactory; whereas if a slit 0.004 inch in width had been employed, it would have been difficult to compare the density given by this slit and one, say, of 0.0005 inch (0.013 mm) on account of the great difference in the purity of the two spectra. An exposure of 30 seconds with slit-width 0.002 inch gave an image of standard density. The relative intensity due to a different slit-width, say 0.001 inch, was determined in the following manner. An exposure of 30 seconds with slit of 0.002 inch was first given; the plate was then moved at right angles to the length of the spectrum, and exposures of 63, 64, and 65 seconds, respectively, were given with a slit-width of 0.001 inch; then the standard exposure of 30 seconds with slit of 0.002 inch; next exposures of 66, 67, and 68 seconds with slit of 0.001 inch; and finally the standard exposure. In this way six exposures with the slit-width for which the intensity is desired, and three exposures with the standard slit, were obtained upon the same plate. From these the exposure for a slit of 0.001 inch, required to give the same density as that for a slit of 0.002 inch and 30 seconds exposure, was selected. Five similar plates were taken for each slit-width. The resulting exposures to the nearest half second, relative to a slit of 0.002 inch and 30 seconds exposure

(for λ 4500, the center of the plate in the Mills spectrograph). are given in the following table:

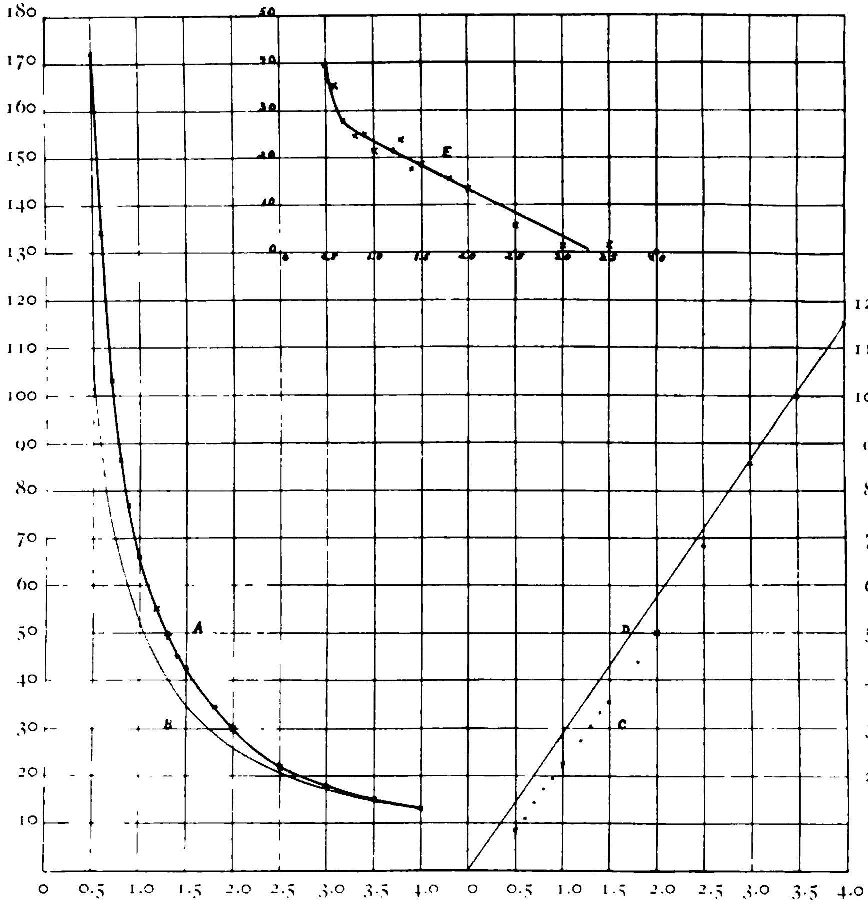
Slit-Width in Units of 0.001 Inch	Time of Exposure in Seconds					Mean Exposure-Time
4.0	13	13	13	12	13	13.0
3.5	15	15	15	16	15	15.0
3.0	18	17	17	18	18	17.5
2.5	22	22	21	22	22	22.0
2.0	...	...	...	...	...	30.0
1.8	35	35	34	34	34	34.5
1.5	43	42	43	42	43	42.5
1.4	45	46	45	45	45	45.0
1.3	50	50	49	49	51	49.5
1.2	55	55	56	55	55	55.0
1.0	66	66	65	66	67	66.0
0.9	77	78	77	77	76	77.0
0.8	85	88	86	87	87	86.5
0.7	104	102	101	105	103	103.0
0.6	132	133	136	133	135	134.0
0.5	175	170	172	172	170	172 0

These values are plotted in Curve A, in which slit-widths in 0.001 inch are the abscissæ, and exposure-times in seconds are ordinates. Curve B is drawn on the assumption that no light is lost by diffraction at a slit of 0.004 inch, and that the exposure-time varies inversely as the width of slit. Curve C represents the variation of photographic intensity with slit-aperture. It was obtained from A by assuming an arbitrary intensity (50) for a slit of 0.002 inch and taking the inverse ratio of the exposure-times. The straight line D represents the intensities on the assumption that they are proportional to slit-width, and that no loss of light by diffraction occurs at a slit of 0.004 inch. The difference in the ordinates of D and C represents the amount of light lost by diffraction. The percentage loss by diffraction for slits of different apertures is shown in curve E, computed on the assumption of no loss at a slit of 0.004 inch.[1]

It will be seen from Curve E that the loss by diffraction in the present experiments increases very rapidly when the slit-width is diminished below 0.001 inch and amounts to about 40 per cent. for

[1] In the present experiments the results for slit-widths from 0.003 to 0.004 inch cannot be determined so accurately as for smaller slit-widths. It is therefore difficult to determine at what slit-width the effect of diffraction begins to enter appreciably.

a slit of 0.0005 inch. The effect of diminishing slit-width is to increase the diameter of the principal maximum of the diffraction pattern and to throw the secondary maximum rapidly away from the axis of collimation. If we assume that the light incident upon the



slit is parallel (as may be permissible for the very small central section of the beam), then from the formula $x = \frac{\lambda a}{s}$ (where λ is the wave-length $0.45\,\mu$, a the focal length of the collimator 722.4 mm, s the width of slit, and x the distance from the center of the collimator to the first minimum of the diffraction pattern), we should find that for

a slit of about 0.0007 inch the principal maximum covers the whole diameter of the collimator lens, and any further decrease in slit-width will 'hrow part of the principal maximum off the lens, causing a rapid increase in the loss by diffraction.

In Curve E it will be observed that for a slit of about 0.0007 inch the loss by diffraction begins to increase very rapidly. The question is so complicated, however, by the beams making a slight angle with the normal to the slit, that it is impossible to draw more than a very general inference concerning the shape of the curve.

By reference to the curve given by Abbot[1] for $\lambda\,1.8\,\mu$ and slit-widths from 0 to 0.5 mm, it will be seen that the percentage loss by diffraction is relatively much greater than that obtained here for $\lambda=0.45\,\mu$ and slit-widths from 0.01 mm to 0.1 mm. A little consideration will show that the discrepancy (aside from the fact that the wave-length employed was four times that used here) is due to the difference in the angular apertures of the collimators employed in the two cases. The one used by Abbot has only about one-fifth the angular aperture of the collimator of the Mills spectrograph. It would therefore appear that when a slit of about 0.3 mm was reached, the principal maximum covered his collimator mirror; and for smaller slit-widths the loss by diffraction increased very rapidly—corresponding to the region of the Curve E for Mills spectrograph slit-widths less than about 0.001 inch.

With the Mills spectrograph, in line-of-sight work, a slit of linear aperture 0.0013 inch (0.033 mm) is found to give sufficient purity. Now, a collimator lens of twice the diameter and focal length of the present one (neglecting the increased absorption of such a lens) would theoretically enable us to utilize about 2.3 times as much light, as we should then be able to double the slit-width. This corresponds to an increase of nearly one magnitude. That is, while we are practically limited now, with the Mills spectrograph, to stars whose photographic magnitude is 5.5, we should then be able to add to the program stars of photographic magnitude 6.5.

On the other hand, the corresponding effective diameter of the collimator lens would be about three inches. It is very doubtful whether it is advisable to use much larger prisms than are employed

[1] *Loc. cit.*, p. 80.

in the present instrument, for several reasons. To mention only one—such prisms by their greater absorption would reduce greatly the increase of light gained in the above manner. We might be able to substitute a reflection grating for the prisms. There are, however, several obstacles in the way of using a reflection grating in a moving instrument. It is necessary to mount the grating so that it cannot move and yet be uncramped. But the most serious difficulty is that any displacement of the grating produces a corresponding shift in the lines of the spectrum. It would be very interesting to compare the loss of light from Rowland's plane gratings with that lost by absorption and reflection in the prisms of the Mills spectrograph, but no data are at hand for such purposes.

We may also use a telescope and collimator of great relative focal lengths, and thus a collimator lens of small linear aperture. With a Cassegrainian reflector it is possible, as is well known, to obtain a great equivalent focal length. But in this case we encounter the difficulty that for average seeing the star image would be larger for the instrument of greater focal length, and while we should gain some light, the gain would not be proportionate.

The telescope and spectrograph of greatest efficiency will be those in which the best compromise is made between the various opposing factors; and this will depend, to a considerable extent, upon the class of work for which the instrument is intended.

Lick Observatory, University of California,
September 5, 1904.

NOTE ON THE LOSS OF LIGHT IN THE 36-INCH LICK OBJECTIVE.[1]

By J. H. Moore.

Since there are no available experimental data for the loss of light by absorption and reflection at the objective of the thirty-six-inch refractor, the present investigation was undertaken with a view to supplying these; and, in particular, for the rays in the region λ 4500.

The visual objective consists of two lenses, a brief description of which, as furnished by Alvan Clark & Sons, is herewith included.

Lens	Radii of Curvature[2]	Thickness		Refractive Index[2] at 20°C., and Barometer 30 Inches
		Center	Edge	
Crown 1734.	$R_1 = +250.52$ in. $R_2 = +250.52$ in.	1.96 in.	0.60 in.	1.520316 ± 2.2 $H\beta$. 1.525677 ± 2.2 G.
Flint 1588.	$R_3 = -230.59$ in. $R_4 = -40000.$ in.	0.03 in.	1.65 in.	1.641340 ± 1.8 $H\beta$. 1.652674 ± 1.0 G.

It will be seen that the kind of glass here employed is quite similar to that of the Potsdam objective. We are, therefore, justified in using Professor Vogel's[3] values for the loss of light by absorption and reflection in lenses of different thickness. Assuming that we have a total thickness of glass of three inches (76 mm), there would result from Vogel's table that the loss caused by the thirty-six-inch objective for the visual rays should be about 27 per cent., and for the actinic rays 40 per cent.

In spectrographic work a thin double-concave lens of 2.5 inches (64 mm) aperture is placed one meter within the visual focus of the telescope, in order to remove chromatic aberration for the region λ 4300–4600. The loss in this lens is estimated to be about 10 per cent.

[1] Also to appear as a *Bulletin* of the Lick Observatory.

[2] *Publications of the Lick Observatory*, **3**, 1, 140, 1894.

[3] Astrophysical Journal, **5**, 80, 1897.

The present determination of the loss for rays at λ 4500 is for the combined objective and correcting lens. The method employed depends upon the following principle. It is easily shown that if the telescope and spectrograph (whose angular aperture is equal to that of the object-glass) is directed toward a surface of uniform illumination, the intensity of the light incident upon the collimator lens would be greater, were the objective and correcting lens removed, by an amount equal to the loss at the object-glass and correcting lens.

The Mills spectrograph, mounted upon the large telescope, was pointed to the region of sky at zero hour-angle and 87° declination (*S. P.*) and a spectrogram taken with slit-width 0.005 inch (0.13 mm) and exposure of 100 seconds. A graduated series of exposures, with the same slit-width, of 45 to 56 seconds, was then given, with spectroscope off the telescope and directed toward the same region of the sky. The length of exposure when the spectrograph is off the telescope (51 seconds from the mean of ten plates) which gives the same density of image as 100 seconds with it on the telescope, represents the percentage of light transmitted by the lenses. It is possible to detect a 2 per cent. variation of the intensity of the images compared, which is sufficient in the present experiments, as errors of this amount may easily enter, due to the variation in intensity of the sky light with changing hour-angle of the Sun. To eliminate this error as much as possible, the exposures were made in the following order: one on telescope; three off telescope; one on telescope; three off telescope; one on telescope. The collimation of the spectrograph mounted on the telescope was checked frequently during the course of the experiments and found to be good. When the spectrograph (off the telescope) was directed toward the sky, light from a surface of greater angular aperture than that of the object-glass would also fall upon the collimator lens, due to diffraction at the slit. In the present experiments any error due to this effect would be very small, as a wide slit was used. It was also shown by another series of tests, in which a slit-width of 0.0006 inch (0.015 mm) was used, that the error introduced in this way by diffraction at the slit was within the errors of experiment. A series of nine plates with slit-width 0.0006 inch gave the percentage of transmitted light as about 52 per cent., in

comparison with 51 per cent. obtained with a slit-width of 0.005 inch (0.127 mm).

The loss by absorption and reflection in the thirty-six-inch objective and correcting lens, for rays of λ 4500, is then 49 per cent. If we take the loss in the correcting lens as about 10 per cent., the value here obtained agrees very well with the value calculated above for the loss in the thirty-six-inch lens for photographic rays.

LICK OBSERVATORY,
September 9, 1904.

Minor Contributions and Notes.

NOTE CONCERNING THE PUBLICATION OF THE CROSSLEY REFLECTOR PHOTOGRAPHS OF NEBULÆ AND STAR CLUSTERS.[1]

THE late Director Keeler's observing program for the Crossley Reflector included the photography of about one hundred of the principal nebulæ and star clusters. The portions of his program available for observation in our clear summer weather were practically complete at the time of his death; but those in position during the cloudy winter months, forming nearly a half of the whole, were incomplete. After the lamented death of Professor Keeler, Assistant Astronomer Perrine, in charge of the Crossley Reflector, made it his first duty to complete this observing program. This was accomplished in September 1903. The importance of prompt publication of this invaluable series of photographs has been fully realized, but difficulties, both technical and financial, have existed. Plans have recently been completed whereby it is hoped to issue, within the coming half year, a volume of Lick Observatory *Publications* to contain high-class reproductions of seventy-two of the principal subjects, as well as a list of several hundred new nebulæ incidentally recorded on the negatives.

The purpose of this announcement is that suitable acknowledgment may be made concerning the generosity of the following friends of the Lick Observatory, University of California, who have provided funds to meet such portions of the expenses of reproducing the photographs as cannot be supplied from printing funds appropriated by the State of California.

Mr. William Alvord.	Mr. Robert Bruce.
Mr. E. J. DeSabla.	Mrs. Phœbe A. Hearst.
Mr. John B. Jackson.	Mr. E. J. Molera.
Miss Matilda H. Smith.	Mr. F. M. Smith.
Miss Jennie Smith.	Mrs. William Thaw.
Mr. Benjamin Thaw.	Mr. Robert J. Tobin.

W. W. CAMPBELL.

AUGUST 3, 1904.

[1] From *Lick Observatory Bulletin* No. 59.

RADIAL VELOCITY OF *T VULPECULAE*.

Two SPECTROGRAMS which I have recently obtained with the Bruce spectrograph show that this short-period variable star (of the type of *δ Cephei*) also varies in its radial velocity—as was to be expected. The plates were both taken with the dispersion of a single prism. The number of lines available for measurement in faint stars like this, when of the solar type, compensates in some degree for the lower dispersion. The results are as follows:

T Vulpeculae ($\alpha = 20^h\ 47^m.2$; $\delta = +27°\ 53'$; Mag. = 5.5 to 6.5).

Plate	Date	G. M. T.	No. Lines	Radial Velocity
IB 379	1904, July 19	$20^h\ 5^m$	9	+15 km
385	22	19 47	13	−17

The predicted phases of the star's light-variation are: Minimum, July $21^d\ 4^h$; maximum, July $22^d\ 13^h$ G. M. T. Unfavorable weather has so far prevented further observations.

EDWIN B. FROST.

YERKES OBSERVATORY,
October 14, 1904.

A NEW *ALGOL VARIABLE*, −15°.4905.[1]

IN the course of an examination of plates in the region of *Sagittarius*, a star which usually appeared to be constant in brightness was noticed by Miss Leavitt to be half a magnitude fainter than usual on one of the plates. Additional plates were examined, and the star was found to be a variable of the *Algol* type. Over three hundred plates were available for the study of the new object, on twenty-eight of which it is fainter than the normal brightness, magnitude 9.55. In three cases it appears faint on two plates taken during the same night, so that twenty-five different minima have been observed. The observations indicate that the times of minimum may be represented by the formula J. D. 2,410,002.677 + 3.45348 E. An interesting feature in the variation is found in the fact that a secondary minimum occurs midway between the primary minima represented by the formula.

The observations when the variable was faint are given in Table I, in which seven plates, taken during the secondary minimum which occurred on the night of September 28, are included. The first three columns give

[1] *Harvard College Observatory Circular* No. 88.

the year, month, and day on which the plate was taken, the Greenwich Mean Time of the center of the exposure, and the Julian Day and fraction following Greenwich Mean Noon. The fourth column contains the observed magnitude, and the fifth the number of the epoch. The sixth column gives the residual found by subtracting the computed time of minimum from the observed time, secondary minima being inclosed in parentheses. The last column gives the residual expressed in magnitudes from the assumed light-curve.

The majority of the observations satisfy the adopted formula very well, especially when it is considered that on a large proportion of the plates the variable is near the edge, so that the observations are somewhat uncertain.

TABLE I.

Observations Near Minimum.

Date	G. M. T.	J. D.	Mag.	Epoch	Phase	Resid.
y m d	h m					
1889 7 6	15 26	1190.643	10.37	344	−0.031	−0.08
1890 8 2	14 13	1582.592	9.85	457	(−0.052)	+0.02
1893 5 1	20 54	2585.871	10.31	748	−0.009	−0.20
1893 5 1	21 8	2585.881	10.51	748	+0.001	−0.04
1895 5 29	19 50	3343.826	9.97	967	(−0.092)	+0.17
1896 7 1	16 19	3742.680	10.11	1083	−0.116	+0.10
1896 10 13	12 21	3846.515	9.81	1113	+0.115	−0.18
1897 6 1	19 43	4077.822	10.58	1180	+0.039	+0.03
1897 8 30	12 38	4167.526	< 10.2	1206	−0.048	......
1898 10 8	12 35	4571.524	9.99	1323	−0.107	−0.07
1899 6 10	18 43	4816.780	10.45	1394	−0.048	+0.07
1899 7 25	15 10	4861.632	10.26	1407	−0.091	+0.08
1899 9 27	12 31	4925.522	9.79	1425	(−0.091)	± 0.00
1900 5 11	19 56	5151.831	10.63	1491	+0.015	+0.08
1900 5 18	20 13	5158.842	9.92	1493	+0.119	−0.04
1901 6 21	14 49	5557.617	9.78	1608	(+0.017)	−0.05
1901 6 26	14 49	5562.739	10.11	1610	−0.041	−0.30
1901 7 17	15 10	5583.632	10.19	1616	+0.131	+0.41
1902 5 15	17 29	5885.730	9.72	1703	(+0.050	−0.02
1902 5 20	19 45	5890.823	11.12	1705	−0.037	+0.70
1902 5 27	16 45	5897.698	10.09	1707	−0.069	−0.18
1902 5 27	19 27	5897.810	10.39	1707	+0.043	−0.15
1902 7 11	16 51	5942.702	10.41	1720	+0.039	−0.15
1902 8 25	12 37	5987.526	10.40	1733	−0.032	−0.05
1903 7 12	15 25	6308.642	10.17	1826	0.089	± 0.00
1903 7 12	16 33	6308.690	10.17	1826	−0.041	−0.24
1904 6 11	18 40	6643.778	10.77	1923	+0.059	+0.27
1904 6 18	16 59	6650.708	10.29	1925	+0.082	−0.06
1904 9 28	12 20	6752.514	9.69	1954	(+0.010)	−0.15
1904 9 28	12 32	6752.522	9.73	1954	(+0.019)	0.10
1904 9 28	12 45	6752.531	9.68	1954	(+0.027)	−0.13
1904 9 28	12 56	6752.539	9.71	1954	(+0.035)	−0.08
1904 9 28	13 8	6752.547	9.72	1954	(+0.043)	0.05
1904 9 28	13 21	6752.556	9.81	1954	(+0.052)	+0.04
1904 9 28	13 34	6752.565	9.73	1954	(+0.061)	± 0.00

On J. D. 5224.578, corresponding to the phase +0.240, the observed magnitude was 9.85. Although the variable is near the edge of the plate, the image is good, and appears to be actually faint. This observation, if correct, suggests a longer duration of the eclipse than is indicated by the other observations. The image on the plate taken on J. D. 5890.823 appears to be defective, but the variable is certainly faint.

The comparison stars employed are given in Table II. The successive columns give the designation, the catalogue number, the uncorrected rectangular co-ordinates, expressed in seconds of arc and referred to the variable as an origin, and the adopted photographic magnitude.

TABLE II.

Comparison Stars.

Des.	*B. D.*	*x*	*y*	Mag.
a.........	−15°4908	+ 358″8	−738″0	9.00
b.........	−15.4906	− 9.0	−498.0	9.42
c.........	−15.4901	− 405.0	+106.2	9.67
d.........	−15.4903	− 252.0	−472.8	9.89
e.........	....	+ 612.0	−682.8	10.49
f.........	−15.4914	+1251.0	−363.6	10.77
g.........	....	− 306.0	−103.8	11.32

Although the time in which the variable can be observed during the present year is short, it is hoped that sufficient observations may be secured to indicate what corrections should be applied to the above formula. An ephemeris is therefore given in Table III. The two columns contain the dates and the times following Greenwich Mean Noon of the primary and secondary minima, respectively. After December 10 the variable will be too near the Sun for observation.

TABLE III.

Ephemeris for Minima.

Prim. Min. 1904	Second. Min. 1904	Prim. Min. 1904	Second. Min. 1904
d h m	d h m	d h m	d h m
Oct. 31 7 30	Nov. 2 0 56	Nov. 21 0 48	Nov. 22 18 14
Nov. 3 18 21	Nov. 5 11 48	Nov. 24 11 40	Nov. 26 5 7
Nov. 7 5 16	Nov. 8 22 41	Nov. 27 22 34	Nov. 29 16 0
Nov. 10 16 7	Nov. 12 9 34	Dec. 1 9 26	Dec. 3 2 53
Nov. 14 3 2	Nov. 15 20 27	Dec. 4 20 19	Dec. 6 13 46
Nov. 17 13 54	Nov. 19 7 20	Dec. 8 7 12	Dec. 10 0 39

This variable star is of interest since its light-curve is intermediate between that of *Y Cygni* and *Z Herculis*. The secondary minimum of the first of these stars is nearly as marked as the primary minimum. The secondary minimum of *Z Herculis* is barely perceptible, and does not differ from full brightness by much more than a tenth of a magnitude. This difference in the case of $-15^{\circ}.4905$ is about three-tenths of a magnitude.

EDWARD C. PICKERING.

OCTOBER 7, 1904.

NOTICE.

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention will be given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed.

Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

If a request is sent *with the manuscript* one hundred reprint copies of each paper, bound in covers, will be furnished free of charge to the author. Additional copies may be obtained at cost price. No reprints can be sent unless a request for them is received before the JOURNAL goes to press.

The editors do not hold themselves responsible for opinions expressed by contributors.

The ASTROPHYSICAL JOURNAL is published monthly except in February and August. The annual subscription price for the United States, Canada, and Mexico is $4.00; for other countries in the Postal Union it is 18 shillings, 6 pence. Correspondence relating to subscriptions and advertisements should be addressed to *The University of Chicago, University Press Division, Chicago, Ill.*

Wm. Wesley & Sons, 28 Essex St., Strand, London, are sole European agents, and to them all European subscriptions should be addressed.

All papers for publication and correspondence relating to contributions and exchanges should be addressed to *Editors of the* ASTROPHYSICAL JOURNAL, *Yerkes Observatory, Williams Bay, Wisconsin, U. S. A.*

THE
ASTROPHYSICAL JOURNAL
AN INTERNATIONAL REVIEW OF SPECTROSCOPY AND ASTRONOMICAL PHYSICS

VOLUME XX DECEMBER 1904 NUMBER 5

INTERNATIONAL CO-OPERATION IN SOLAR RESEARCH.

MINUTES OF THE MEETING OF DELEGATES TO THE CONFERENCE ON SOLAR RESEARCH, HELD IN THE HALL OF CONGRESSES, ST. LOUIS, SEPTEMBER 23, 1904.

As CHAIRMAN of the Committee on Solar Research of the National Academy of Sciences, which had issued the call for the Conference, Professor George E. Hale called the meeting to order and explained the purpose of the Conference. After referring to previous movements to secure co-operation in solar research, he emphasized the importance of encouraging individual initiative, and urged that no less attention be paid to such encouragement than to the accomplishment of large pieces of routine work through co-operative effort.[1]

The following officers were then elected to serve for this meeting:

President Professor George E. Hale.
Vice-President Professor Henri Poincaré.
Recording Secretary Professor C. D. Perrine.

A motion that for this meeting the voting be by societies, and that each society be allowed one vote, was adopted.

A motion was made and carried that the chairman of each society's committee should cast the vote for that society.

After some remarks by Professor Hale on co-operation, the following motion was made by Professor Turner:

[1] See ASTROPHYSICAL JOURNAL, December 1904.

That this meeting is in favor of the organization of a scheme of international co-operation in solar research which shall encourage individual initiative, provide suggestions for definite lines of work, and facilitate the collection of results for publication.

This expression, after remarks by Professors Poincaré, Boltzmann, and Hale, was made the sense of the meeting.

Professor Hale stated that in appointing its Committee on Solar Research the National Academy of Sciences had given the committee authority to join the president and foreign secretary of the Academy in presenting its plans for co-operation to the International Association of Academies. The opportunity now existed of securing the views of the Conference on this subject.

After remarks by various delegates upon the relations of scientific societies among themselves and to the International Association of Academies, the following motion was made by Professor Poincaré:

That the committee to be appointed at this meeting negotiate with the Academy of Sciences of Vienna with the view of obtaining the approval and the patronage of the International Association of Academies.

This motion was seconded by Professor Boltzmann and adopted.

A short discussion then ensued upon the fulness of the representation of interested individuals and societies in the preparation of plans for co-operation in solar research.

Dr. Bauer moved:

That the subcommittee on Terrestrial Magnetism and Atmospheric Electricity of the International Meteorological Committee be invited to appoint a committee to co-operate with the Solar Research Committee.

This motion was seconded by Mr. Rotch and adopted.

Professor Turner moved:

That the Hungarian Academy of Sciences of Budapest also be invited to appoint a committee to co-operate with the Solar Research Committee.

This motion was seconded by Professor Frost and adopted.

The following general motion was then made by Professor Poincaré and adopted:

That the International Committee on Solar Research, to be appointed, be authorized to invite, at its discretion, societies and individuals which have been omitted, to co-operate.

The subject of the formation of an international committee was then discussed.

Professor Turner moved:

That each committee participating be invited to appoint a member to serve on the International Committee.

This motion was seconded by Professor Campbell and adopted.

Mr. Rotch, delegate of the subcommittee of the International Meteorological Committee on the relationship between meteorological and solar phenomena, reported on the recent meeting of the subcommittee at Cambridge. At that meeting the invitation of the National Academy of Sciences to co-operate with the other committees was accepted.

A short discussion followed on a provisional program of observations, at the conclusion of which Professor Turner nominated the following gentlemen as a committee to prepare a provisional program, such committee to have power to add to its number at discretion: Professor George E. Hale, Professor Arthur Schuster, Professor Svante Arrhenius.

Suggestions from M. Deslandres as to observations to be undertaken, and the adoption of certain names of instruments and solar phenomena, were, at the suggestion of Professor Poincaré, referred to the Committee on Program.

Following a discussion of routine measurements and computation, Professor Turner offered, on behalf of the Oxford University Observatory, to undertake a portion of this work.

A motion was therefore made by Mr. Abbot that the International Committee enter into negotiations with the Oxford University Observatory with the view of providing for the measurement of plates and the reduction of solar observations.

This motion was seconded by Professor Campbell and adopted.

The suggestion was made by Dr. Bauer that if visual magnetic observations, especially magnetic observations at the eclipse of 1905, could be made at observatories with special instruments provided for the purpose, it was practically certain that the reduction of such observations would be undertaken by the Carnegie Institution.

A memoir on standards of wave-length, prepared by MM. Pérot and Fabry on behalf of the Physical Society of France, was presented by Professor Poincaré.[1]

A motion that standards of wave-length should be determined

[1] Printed elsewhere in this number.—EDS.

from terrestrial instead of solar sources was referred for consideration to the International Committee.

Professor Crew discussed the question of standards of wavelength, and pointed out the necessity of replacing Rowland's standards by a new system. He also presented a paper on this subject by Professor Kayser and a letter from Professor Michelson.

Mr. Jewell presented in abstract a paper on Rowland's standards.

Following is a list of committees and delegates appointed by various societies. Other societies will appoint committees in the near future:

Society	Committee	Delegates Present at Conference
Royal Society of England....	Huggins, Christie, Lockyer, Schuster, Newall	Turner
Royal Astronomical Society..	Turner, and others to be appointed	Turner
Astronomical Society of France....................	Deslandres, de la Baume Pluvinel, Guillaume, and others not yet named	Poincaré
French Physical Society......	Poincaré, Fabry, Pérot, Deslandres, and others not yet named	Poincaré
German Physical Society....	Ebert, Kayser, Kreusler, Lummer, Pringsheim, Runge, Straubel, Wilsing	
Amsterdam Academy of Sciences	Kapteyn, Julius	Kapteyn
Society of Italian Spectroscopists....................	Tacchini, Riccò, Chistoni, Mascari	
Vienna Academy of Sciences.	Hann, Weiss, von Lang, Exner	Boltzmann
St. Petersburg Academy of Sciences....................	Backlund, and others not yet named	Backlund
Subcommittee of International Meteorological Committee....		Rotch
Stockholm Academy of Sciences..................		Arrhenius[1]
Astronomical and Astrophysical Society of America......	Frost, Abbot, Bauer, Jewell, Perrine	Frost, Abbot, Bauer, Jewell Perrine
American Physical Society..	Ames, Crew, Lewis, Mendenhall, E. F. Nichols	Crew, Mendenhall
National Academy of Sciences	Hale, Campbell, Langley, Michelson, Young	Hale, Campbell

[1] Acting informally.

A motion by Professor Poincaré was then adopted referring these communications to the International Committee.

Owing to lack of time for discussion, the subject of total solar eclipses was referred to the International Committee, upon motion of Professor Campbell.

Oxford and Meudon were suggested as places for the next meeting. The International Committee was empowered to decide both the place and time of meeting, on motion of Professor Frost, seconded by Professor Poincaré.

Upon motion of Professor Poincaré, the thanks of the Conference were tendered to Professor Hale for his interest and activity in connection with the Conference.

The Conference then adjourned.

GEORGE E. HALE, *President.*

C. D. PERRINE, *Recording Secretary.*

CO-OPERATION IN SOLAR RESEARCH.[1]

By George E. Hale.

We have come together, gentlemen, in response to an invitation from a committee of the National Academy of Sciences to consider the possibility of inaugurating a plan of international co-operation in solar research. Such a movement cannot be considered an entirely new one; for in the history of astrophysics we find records of several undertakings of a similar nature, some of which have yielded results of great importance. Foremost among these, perhaps, should be classed the important work of the Society of Italian Spectroscopists, which, through the observations of its own members and the effect of their example upon others, has added in a high degree to our knowledge of the Sun. We should also remember the important work inaugurated by Wolf and ably continued by his successor, Wolfer, at Zurich, of compiling Sun-spot numbers from observations made in all parts of the world. The well-known series of solar photographs discussed at Greenwich is another instance of a similar kind; for the negatives are secured through the co-operation of the Royal Observatory at Greenwich, the Solar Physics Observatory at Dehra Dun, in India, and the Observatory at Mauritius. These photographs constitute the most important record of Sun-spot statistics now available. Indeed, they are so complete as to render similar work unnecessary, so far as the duplication of the extensive reductions go, on the part of other observatories. The initiation of this series of photographs is due to the Solar Physics Committee appointed by the British government in 1879. In 1885 this committee proposed to hold an international conference in London to consider plans for co-operation in solar research; but through the inability of delegates to attend the conference the project seems to have been abandoned. At the congress held in Paris, in 1887, to prepare plans for making the International Photographic Chart of the heavens, there was some discussion of inaugurating co-operative investigations of an astro-

[1] Introductory remarks of the chairman of the Committee on Solar Research of the National Academy of Sciences.

physical nature; but hitherto they have not been carried into effect. More recently, a solar commission appointed by the Astronomical Society of France recommended that a general plan of solar research be undertaken. So far as I am aware, however, the work of this solar commission has hitherto been confined to France. At the recent meeting of the British Association in Cambridge a subcommittee appointed by the International Meteorological Committee met to consider "the combination and discussion of meteorological observations from the point of view of their relations with solar physics." The principal object of this movement, as will be seen from the announcement of the purpose of the committee, is to deal with meteorological questions related to solar phenomena. In the consideration of purely solar questions, I am happy to say that this committee will co-operate with the other committees represented at the present conference.

From this brief, and avowedly incomplete, historical sketch it will be seen that the movement we are inaugurating today can lay no claim to novelty. It is merely the culmination of a natural series of events, each of which has tended to emphasize the importance of solar research and the desirability of harmonious co-operation among the investigators at work in this field. I have had frequent occasion to point out the great possibilities which lie open to the observer of solar phenomena. It may be said without exaggeration that no department of science, so far as I am able to judge, offers more promising opportunities for important developments. In almost all cases solar observations are now being made with the methods and apparatus of a quarter of a century ago, and the powerful instruments of modern times, both astronomical and physical, have been but little applied in solar research. I do not mean that this is a rule without exceptions, but it nevertheless applies in a large proportion of cases. There can be no question that great advances in our knowledge of the Sun merely await the application of instruments and methods already available, but hitherto rarely employed.

So far I have spoken with special reference to observations of solar phenomena, but there are other questions of general spectroscopy which should play a no less important part in the deliberations of this conference. No one who is acquainted with the progress of

spectroscopy during the last twenty-five years in the physical laboratory can deny that its methods and its problems should be considered at the same time with the problems of solar physics. Within a very short time laboratory experiments on the phenomena of anomalous dispersion have yielded results so striking as to lead certain investigators to interpret solar observations on an entirely new basis. Although most solar physicists will doubtless hesitate to accept the radical changes in solar theory which have been advanced, they must nevertheless be prepared to give careful consideration to the new claims. I might continue by pointing to many other phenomena of the laboratory, which must be studied in the closest union with solar questions; but this is needless before the present company. One question of the greatest importance—that of standard wave-lengths—will be fully treated by Professor Crew, and therefore need not be discussed by me. In my opinion, if I may be permitted to express it, the discussion of this and other similar subjects at the present conference should be regarded as preliminary, subject to further consideration by the various committees before final action is taken.

Two fundamentally different views are entertained among astronomers regarding the desirability of inaugurating schemes of co-operation in research. On the one hand, there are those who strongly favor co-operative undertakings, on the ground that through the intelligent distribution of work, which such a scheme assumes, large investigations can be carried out with the minimum expenditure of time and effort. Many who hold this view frankly assume that the participants in a co operative undertaking are to perform their respective parts like so many machines, following a hard-and-fast program from which no deviation can be permitted. This system involves the existence of a central bureau which exercises control over all of the work and, in some cases, carries out the extensive reductions incidental to the research. This view is opposed by a large number of investigators, who think that the future of science depends in far greater degree upon the development of new ideas and the encouragement of individual genius than upon the accomplishment of any piece of work, no matter how extensive it may be. They urge, and with strong reason, that those who engage in a co-operative undertaking, the lines of which have been sharply defined in advance, leaving no

option to individual participants, must inevitably suffer, in so far as their originality of thought is concerned. Thus, while admitting that important results, perhaps not otherwise obtainable, can be secured by co-operation among individuals and institutions, they maintain that the loss will be greater than the gain in case the effect of the domination of some central authority is such as to stifle individual effort and prevent the development of new ideas and methods.

During the last few years these two phases of the subject have received much discussion in connection with many co-operative undertakings in science, variously organized and developed, and playing an increasingly important part in modern research. In spite of objections urged by some of the ablest investigators, these co-operative undertakings are actually being carried out; some with marked success, and others with less promise as to their future outcome. It must not be forgotten that in different departments of science the nature of the work varies so greatly as to make the question of co-operation apply in very different ways in different cases. In meteorology, for example, it is obviously essential that a great number of institutions should make routine observations at appointed hours and upon a carefully arranged plan, in order that the results may be suitable for systematic reduction and study. The same may be said of many other classes of work, particularly those which involve routine observations of secular phenomena. But it will be recognized that even in such a case there is nothing to preclude the development of new ideas and methods by those who are taking part in such a co-operative investigation. The danger, if any danger exists, is that the individual observers may be allowed to believe that the instruments and methods they are called upon to employ represent the highest possible development, and that any deviation from the scheme must stand in the way of the results it is desired to attain.

In the history of science, instances are not lacking of co-operative undertakings which have dealt injury to participants. Instead of encouraging the development of new ideas, they have allowed the entire stress to be laid upon the accomplishment of given pieces of work according to a strict routine, and, either directly or indirectly, they have certainly discouraged individual effort, and thereby hindered progress. It is with a full appreciation of this fact that I

approach the subject of co-operation in solar research. While I believe unquestionably that a science like our own cannot accomplish the most important advances without the collection of extensive data, beyond the reach of any individual or institution, I nevertheless feel that the future of the science depends in a much higher degree upon the encouragement of individual initiative. In suggesting, with others, the desirability of adopting some general program of solar research, in which the observers and instruments in various parts of the world may find a part, I do so only on the supposition that such a movement need involve no discouragement of individual effort. Indeed, I would say more than this. I am not in favor of entering upon a co-operative undertaking unless it can be so planned as actually to encourage individual effort through the provision of conditions which would otherwise be lacking. Specifically, I believe that one of the principal objects of establishing committees on solar research should be the encouragement of little-known investigators to bring forward and develop ideas that may have originated with them. It may frequently happen that the special atmospheric or instrumental conditions required in the accomplishment of a new piece of work may be readily available at some existing observatory, where they could be placed at the disposal of an investigator who might employ them in the realization of his ideas. Through the work of the various national committees, such cases would easily become known, and, in many instances, important new ideas might thus be advanced which would otherwise remain unknown.

At the very outset of the present undertaking, therefore, I would propose that the encouragement of individual ideas be regarded as one of the most important functions of any organization we may establish. But, while attaching so much weight to this phase of the subject, I see no incompatibility between the encouragement of original ideas and the accomplishment of certain pieces of work which can best be done through co-operation. For example, let us take the observation of the spectra of Sun-spots. No one who has observed the spectrum of a Sun-spot and attempted, map in hand, to identify and record all of the widened lines, is likely to be so ambitious as to desire to include the entire length of the spectrum in his program of daily observations. To secure adequate knowledge

of Sun-spot spectra, it seems to me essential that practically all of the lines affected in the spots should be recorded. Those who share this view would undoubtedly be content to confine their attention to certain limited regions of the spectrum. At the time of the Sun-spot maximum, when many spots must be observed, it is obvious that the extent of the spectrum covered by one observer should not be too great. Furthermore, on account of interruptions from bad weather and the desirability of securing a check on the observations, it is equally obvious that the same region of the spectrum should be observed regularly at more than one institution. Thus our knowledge of the spectra of Sun-spots would undoubtedly be materially increased if an understanding were reached as to the division of the spectrum among different investigators. From my knowledge of the work of this kind now in progress, or contemplated, I would anticipate no difficulty in securing such a division of labor. I can see no reason why a mutual understanding on this subject could be otherwise than advantageous to all who are interested in solar research.

In this reference to an important department of solar physics, which has been sadly neglected up to the present time, I have purposely confined my remarks to pure routine observations, such as are involved in the recording of the widened lines. But in this same field of research there is almost unlimited opportunity for individual investigations, many of which might involve the application of new and original ideas upon the part of those who pursue them. Hitherto, strangely enough, little or no attention has been given to the quantitative side of this subject, and few records of the carefully measured positions of widened lines can be found in the literature of astrophysics. Thus we have no knowledge of any such displacement of lines as may be produced by pressure or by the bodily motion in the line of sight of the gas which gives them rise. Line-distortions, it is true, have sometimes been measured; but my present reference is to the less violent motions which may possibly affect the entire mass of gas within the Sun-spot. There is large opportunity, also, for observations of the fine lines into which Young resolved the spot-band, as well as the interesting reversals so recently recorded by Mr. Mitchell, working under Professor Young's direction at Princeton. What I have said relates simply to visual observations, which must

continue to play a very important part in the study of spot-spectra. There is a great opportunity, however, for the development of photographic methods in this connection, and here, again, there will be abundant opportunity for the individual explorer.

This single instance will serve at once to illustrate the desirability of co-operation in solar work and the possibility of securing a system of co-operation which will accomplish the desired purpose and yet leave the individual free to initiate new classes of work. Numerous similar instances might equally well be mentioned. In all cases the development of the method by those who take part in the work should be encouraged in every possible way.

I have dealt at such length with this question because it is so frequently assumed that co-operative work must injure the individual observer. I believe, on the contrary, that a system can be devised which, through co-operative effort, will result in an increase in the number of original suggestions and a more rapid improvement of observational methods.

REMARKS ON STANDARD WAVE-LENGTHS.[1]

By HENRY CREW.

It is a matter of no little interest that the man who laid the foundations of mathematical astronomy and devised the corpuscular theory should also have been the first to give us an absolute measure of the wave-length of light. Newton's values, published in 1704, remained unsurpassed for more than a century.

In 1815, Fraunhofer employed his newly invented gratings to determine some of the principal solar lines; these remained standard for nearly half a century.

Kirchhoff in 1862 gave us his beautiful prismatic map of the solar spectrum, a model of precision and care.

The first normal map of the solar spectrum was published by Ångström in 1868. It was based upon his own determinations of absolute wave-length and was printed on a scale such that one millimeter on the map indicates a difference of one tenth-meter in wave-length. For twenty years this work stood in the front rank. But in the later eighties there began to appear measurements of both solar and metallic spectra which introduced an order of accuracy hitherto undreamed of. Thalén, in 1884, pointed out the numerous sources of error in Ångström's map and measures.

But the year 1888 marks the beginning of a new era. For it was at this date that Rowland put out the final edition of his superb photographic map of the solar spectrum, and it was in the same year that Kayser and Runge gave us their exquisite rendering of the spectrum of the iron arc, both of these maps being based upon the absolute wave-length of the D lines, which Bell had just completed with extraordinary care by the grating method. A short list of solar standards was published by Rowland at the same time.

I remember very distinctly the general feeling in the spectroscopic circle which I then knew that, with the publication of Rowland's map, accompanied by his relative wave-lengths, and Bell's absolute

[1] Introductory remarks on opening the discussion on this topic at the solar conference.

value, we were at last on solid ground where a stand of many years might be made.

On the contrary, in 1893, Michelson had—by a method as yet unimpeached—already translated the standard meter into terms of the red cadmium ray, and had incidentally proved that Bell's value of the absolute wave-length was too large by something like two-tenths of an Ångström unit.

Since then Michelson's values have been confirmed as perfectly as possible—at least, without the introduction of a totally different method—by Fabry and Pérot, and Hamy.

In this same year, 1893, appeared Rowland's "New Table of Standard Wave-lengths," including both metallic and solar lines, upon which Hasselberg has based his highly accurate series of metallic arc spectra, starting with chromium in 1894.

In 1895 Rowland began, with the first number of the ASTROPHYSICAL JOURNAL, the publication of what he modestly called a "Preliminary Table of Solar Spectrum Wave-Lengths," expecting, as he says, "to add to it and correct it for a term of years until I can publish a standard list of the lines of the solar spectrum with all the elements to which they belong." From the very beginning the wide divergence between this table and the more probably correct absolute values of Michelson was admitted. The relative values of the table appeared for the time being almost beyond criticism.

In 1896 Jewell[1] published his observations on the want of exact coincidence between solar and corresponding metallic lines, showing not only that the solar lines are displaced in general toward the red but that the different lines of any one element are *differently displaced*. The effect of this was, using Mr. Jewell's own words, "to make the lines of the solar spectrum step down from the commanding position which they have occupied as standards of reference."

For the next four years matters remained about as Jewell's paper left them.

In 1900, however, Kayser discovered certain inconsistencies in Rowland's 1893 table, that is, the "New Table of Standard Wavelengths." He found also that the distribution of iron lines in this table was such as to render it quite inadequate for purposes of accurate

[1] ASTROPHYSICAL JOURNAL, 3, 89, 1896.

interpolation. To use Kayser's own words concerning a certain group of Rowland's iron standards: "My correction curves depending upon these lines always showed a quite impossible bend at λ 3400, so that I was finally compelled to omit these standards."[1] These "impossible bends" in Kayser's curves refer to differences as great as 0.02 tenth-meter. Kayser at the same time published a fuller and revised list of iron standards, extending from λ 2327 to λ 4495, the relative values of which are based upon Rowland's metallic standards and are claimed to have a mean relative error not exceeding 0.003 tenth-meter. In other words, the *accidental* errors are much less than those of Rowland's "New Table of (Metallic) Standards."

The state of affairs at the time of Rowland's death (spring of 1901) may perhaps be described by saying that three shadows had fallen across the superb work embodied in his "Preliminary Table;" namely, (1) the wide divergence from the correct absolute values, shown by Michelson; (2) the uncertainty attaching to solar lines, shown by Jewell; and (3) certain inconsistencies in relative values, shown by Kayser. I feel, however, that the figure is misleading; and with your permission I will change it and say that the subject had received illumination from three new sources.

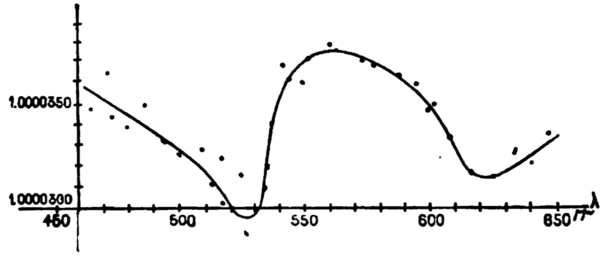
In the year 1902 additional illumination was added from another brilliant source. I refer to the absolute determination of thirty-three solar lines and fourteen iron lines by Pérot and Fabry, using the method of interference. These measures carried with them two important results; namely, (1) they proved that the relative values of the "Preliminary Table" were accurate, over small ranges, to one part in a million; in other words, the curve of errors in this table is a "smooth one;" (2) they proved that the relative values in this table deviate in each direction from the mean axis by as much as 0.02 tenth-meter, that is, by four parts in a million. In short, they proved that the absolute errors in the "Preliminary Table" extend to the first decimal place, the relative errors over any considerable range extend to the second decimal place, while neighboring lines are affected with relative errors only in the third decimal place. The serious consequences of this discovery are realized only when we recall that for seventeen years past all the spectroscopic work of the world

[1] ASTROPHYSICAL JOURNAL, 13, 331, 1901.

has been based upon this system which the two French physicists have shown to be affected with large systematic errors. In line-of-sight work, where 0.01 tenth-meter means 0.7 km per second, the importance of these errors is enormous.

The upshot of the whole matter is that Rowland's relative values are too small from about the F line to the middle of the green, while from the middle of the green to the D lines his values are too large by about the same amount, namely, an error ranging up to 0.02 Ångström unit.

The year 1902 thus brings us to the end of what may be called



indisputable ground, which may be summarized by saying that Rowland's system, possessing these various defects, is still an instrument of great value, though less perfect than we formerly thought. Just how these errors have crept into the system is a matter of no little importance; indeed, it is a matter of very great importance, if we are to avoid similar errors in the future; at the same time, this is not the principal subject under discussion by this conference. Hartmann has set forth[1] in a clear way the various sources of systematic and accidental errors in Rowland's tables. Michelson has predicted, and Kayser has verified, the fact that for gratings in general, the method of coincidences is not reliable even for relative values, except within limits which are much larger than was formerly believed. But the main question before us is not how the imperfections of the system arose; it is rather how the system may be perfected, with a minimum expenditure of time and energy, so as to answer the pur-

[1] "A Revision of Rowland's System of Wave-Lengths," ASTROPHYSICAL JOURNAL, 18, 167–190, October 1903. Reprints of this article were sent by Professor Hartmann for distribution among those participating at the solar conference, as containing the expression of his views on the subject.

poses of modern spectroscopy. Various suggestions looking toward this result have recently appeared. These would perhaps naturally form the subject of our discussion. So far as I am aware, the principal alternatives may be summarized as follows in terms of the accompanying correction curve, which Fabry and Pérot published in the ASTROPHYSICAL JOURNAL, **15,** 272, 1902:

1. Correct to the axis of λ as rapidly as determinations of the grade of those by Michelson, and Pérot and Fabry will allow.

2. Reduce to a line drawn *parallel to the axis* of λ at such a distance that the sum of Rowland's errors will be zero.

3. Reduce to a straight line *intersecting the axis* of λ at such an angle as to make the sum of the squares of Rowland's errors a minimum.

These alternatives leave untouched the question of extending the corrections to the ultra-violet region.

RAPPORT SUR LA NÉCESSITÉ D'ÉTABLIR UN NOUVEAU SYSTÈME DE LONGUEURS D'ONDE ÉTALONS.[1]

Par A. PÉROT et CH. FABRY.

LA SOCIÉTÉ FRANÇAISE DE PHYSIQUE, comme suite à la communication de la National Academy, a nommé une commission chargée d'étudier la question de l'établissement d'une échelle de longueurs d'onde. C'est sur l'invitation de cette commission que nous avons préparé le présent rapport pour le Congrès International de Physique de l'Exposition de Saint Louis.

Nous avons, dans les lignes suivantes, cherché à exposer le plus clairement possible la nécessité de l'établissement d'une échelle de longueurs d'onde, à montrer que le spectre solaire devait être abandonné comme étalon, et à définir l'unité absolue.

La mesure des longueurs d'onde, outre qu'elle est nécessaire aux chimistes pour charactériser les corps par leur spectre d'émission, offre des moyens uniques d'investigation, aux physiciens en particulier pour l'étude de l'émission lumineuse et de la constitution des radiations, aux astronomes pour la mesure des vitesses radiales des astres et la recherche des phénomènes qui se passent à la surface de notre soleil, mais, pour que ces études aient une base certaine, il faut que les longueurs d'onde soient mesurées avec toute la précision voulue, et il est intéressant au point de vue international d'adopter une unité définie qui, une fois choisie, ne devra plus varier.

Dans les recherches spectroscopiques, on est amené généralement à faire des mesures relatives, c'est-à-dire à comparer la longueur d'onde de la radiation étudiée à celle d'une radiation connue; s'il est possible avec certaines méthodes, telle la méthode interférentielle, de comparer directement sans aucun intermédiaire les longueurs d'onde de deux radiations quelconques, la méthode employée d'une manière courante, qu'elle repose soit sur l'emploi de prismes, soit sur celui de réseaux, ne permet que la détermination du rapport des longueurs d'onde de raies relativement voisines. Il résulte de là que la détermination d'une seule longueur d'onde serait insuffisante

[1] Présenté au nom de la Société Française de Physique.

pour les applications, et qu'il faut, si l'on veut faire œuvre utile, mettre entre les mains des spectroscopistes les valeurs d'une série de longueurs d'onde convenablement choisies, déterminées par des méthodes instituées spécialement à cet effet avec toute la précision requise. Il en est de même dans d'autres travaux, tels par exemple que le nivellement du sol: loin de rapporter l'altitude d'une station quelconque directement au niveau de la mer, on relie usuellement l'altitude de cette station à celle d'une autre station déjà connue qui sert de base auxiliaire. On voit donc que, s'il est indispensable de déterminer les sources étalons de manière que les radiations émises puissent toujours être produites identiques à elles-mêmes, il est nécessaire également de fixer les valeurs des longueurs d'onde d'un nombre suffisant de points de repère convenablement choisis.

L'établissement d'un exact système de longueurs d'onde est d'une importance capitale, puisque toute erreur sur ces longueurs d'onde fondamentales se reportera dans toutes les mesures spectroscopiques. Il serait désirable que les erreurs contenues dans le système fondamental fussent inférieures ou au plus égales aux erreurs accidentelles qui peuvent résulter des mesures par comparaison. Or il est à peu près certain qu'il n'en est pas actuellement ainsi: les mesures par comparaison peuvent être faites, avec les puissants appareils dispersifs dont on dispose actuellement, avec une précision qui dépasse le millionième; telle serait la précision des mesures si les valeurs des longueurs d'onde des raies de comparaison avaient la même précision: or celle de l'échelle adoptée est beaucoup moindre; aussi, M. Kayser pouvait-il récemment émettre l'opinion qu'une grande partie des désaccords entre les mesures faites par divers observateurs sur une même raie provenait de l'emploi de valeurs incorrectes pour les longueurs d'onde des raies de comparaison.[1]

Depuis les admirables travaux de Rowland, c'est-à-dire depuis environ 15 ans, tous les travaux de spectroscopie ont été faits en prenant comme point de départ les nombres donnés par ce savant. On pouvait cependant déjà prévoir une difficulté dans l'application

[1] H. KAYSER, ASTROPHYSICAL JOURNAL, **19**, 158, 1904: "With a correct system of standards we could now determine the wave-lengths of all the sharp lines—and 99 per cent. of all the lines can be got sharp—with an accuracy of a few thousandths of an Angström unit. I am sure that the much larger differences found by different observers are caused very often by the use of different, relatively incorrect, standards."

de ce système de longueurs d'onde: les mesures de Rowland ont été faites sur le spectre solaire, il faudrait donc, pour opérer d'une manière correcte, prendre le spectre solaire comme spectre de comparaison, c'est ce que peut-être, aucun observateur n'a fait; on s'est servi de spectres de métaux, en admettant que les longueurs d'onde sont les mêmes que dans le spectre solaire; or l'exactitude de ce postulat est de plus en plus improbable. Il est vrai que certaines raies métalliques, produites par l'arc électrique ont été spécialement mesurées par Rowland, mais ces mesures ne méritent pas la même confiance que celles des raies du spectre solaire; Rowland était convaincu de l'identité absolue des longueurs d'onde dans le spectre solaire et dans celui de l'arc électrique, et lorsque des écarts se manifestaient entre les deux espèces de spectres, il les attribuait à un déplacement de la plaque photographique ou à un défaut de réglage des faisceaux, et il cherchait par des corrections convenables à faire disparaître les écarts dans les résultats.[1] En employant les nombres de Rowland pour des raies de métaux dans l'arc électrique, on emploie donc ou bien des nombres obtenus en mesurant autre chose que ce que l'on emploie, ou bien des nombres un peu incertains; il faudrait se résoudre, si l'on voulait profiter le mieux possible des travaux de Rowland, à employer uniquement le spectre solaire comme spectre de comparaison.

Mais les valeurs données par Rowland pour les longueurs d'onde du spectre solaire sont-elles du moins parfaitement correctes? L'auteur pensait qu'elles étaient exactes au millionième environ. La meilleure vérification consistait à reprendre les mesures d'un certain nombre de raies du spectre solaire lui-même, par une méthode aussi différente que possible de celle de Rowland, et autant que possible plus directe. C'est ce que nous avons fait[2] en comparant directement, par une

[1] ROWLAND, *Physical Papers*, p. 564: "In every plate having a solar and metallic spectrum upon it there is often—indeed always—a slight displacement. This is due either to some slight displacement of the apparatus in changing from one spectrum to the other, or to the fact that the solar and the electric light pass through the slit and fall on the grating differently. In all cases an attempt was made to eliminate it by exposing on the solar spectrum, both before and after the arc, but there still remained a displacement of 1/100 to 2/100 division of Angström, which was determined and corrected for by measuring the difference between the metallic and coinciding solar lines, selecting a great number of them, if possible."

Voir aussi: L. E. JEWELL, ASTROPHYSICAL JOURNAL, **3**, 89, 1896.

[2] CH. FABRY et A. PÉROT, "Mesures de longueurs d'onde en valeur absolue; spectre solaire et spectre du fer," *Annales de Chimie et de Physique*, Janvier 1902; et ASTROPHYSICAL JOURNAL, **15**, 73, 261, 1902.

méthode interférentielle, un certain nombre de raies du spectre solaire avec une même raie du cadmium. Nos mesures ont porté sur la partie du spectre comprise entre les longueurs d'onde 464 et 647.

La comparaison de nos résultats avec ceux de Rowland conduit aux résultats suivants:

1. Il n'existe pas, dans les tables de Rowland, d'erreurs accidentelles atteignant le millionième; il est même probable qu'au point de vue des erreurs accidentelles, les nombres de Rowland sont au moins aussi précis que les nôtres. Si l'on prend dans les tables de Rowland deux radiations très voisines, les rapports des nombres donnés pour les longueurs d'onde sont toujours parfaitement exacts.

2. Il existe dans les mêmes tables des erreurs systématiques notables (atteignant presque le cent-millième); ces erreurs varient d'une façon régulière en fonction de la longueur d'onde. Si donc on prend dans la table de Rowland les longueurs d'onde de deux radiations situées d'une manière quelconque, le rapport des deux nombres peut être erroné presque de 1 cent-millième, c'est-à-dire d'une quantité dix fois plus grande que les erreurs que l'on peut commettre dans les mesures de comparaison.

L'exactitude de nos résultats a donné lieu au début, à quelques contestations, elle semble aujourd'hui généralement admise.[1] On ne voit d'ailleurs pas comment des erreurs systématiques, fonctions de la longueur d'onde, auraient pu s'introduire dans nos mesures.

Il y aurait donc lieu de reprendre les mesures de Rowland. Les erreurs n'étant que systématiques, et régulièrement variables avec la longueur d'onde, on pourrait se proposer de construire une table

[1] Voir à ce sujet:

LOUIS BELL, "On the Discrepancy between Grating and Interference Measurements," ASTROPHYSICAL JOURNAL, **15**, 157, 1902.

A. PÉROT AND CH. FABRY, "A Reply to the Recent Article by Louis Bell," *ibia.*, **16**, 36, 1902.

LOUIS BELL, "The Perot-Fabry Corrections of Rowland's Wave-Lengths," *ibia.*, **18**, 101, 1903.

FABRY ET PÉROT, "On the Corrections to Rowland's Wave-Lengths," *ibid.*, **19**, 119, 1904.

G. EBERHARD, "Systematic Errors in the Wave-Lengths of the Lines of Rowland's Solar Spectrum," *ibid.*, **17**, 141, 1903.

J. HARTMANN, "A Revision of Rowland's System of Wave-Lengths," *ibid.*, **18**, 167, 1903.

H. KAYSER, "On Standards of Wave-Lengths," *ibid.*, **19**, 157, 1904.

de correction, analogue à celle que nous avons donnée, mais plus étendue, de manière à profiter complètement du travail de Rowland; mais il ne faudra pas, dans ce cas, perdre de vue que les nombres de Rowland ne s'appliquent qu'au spectre solaire, et si l'on veut avoir les longueurs d'onde de raies métalliques, il faudra les comparer directement et sans idée préconçue aux raies solaires. On peut se demander s'il ne serait pas plus sûr, et presque aussi simple, de reprendre complètement le travail de la détermination des étalons. Nous allons donc envisager les diverses solutions possibles, en supposant le travail repris par la base.

Choix des radiations.—La première question à résoudre paraît être celle-ci: Convient-il, pour définir l'échelle des longueurs d'onde, d'adopter des radiations empruntées au spectre solaire, ou des radiations d'origine artificielle?

C'est la première de ces solutions que l'on a adoptée au début de la spectroscopie (Ångström), et que Rowland a conservée. Il est certain que l'emploi du spectre solaire a l'avantage de dispenser l'observateur de tout soin à donner aux sources de lumière, et que le nombre immense des raies de son spectre offre dans certains cas des avantages; ce grand nombre de raies était à peu près nécessaire pour l'emploi de la méthode des coincidences de Rowland. Mais l'emploi de ce spectre offre des inconvénients qui contrebalancent, et bien au delà, ces avantages: en dehors des altérations régulières de longueur d'onde, produites par la rotation du soleil et par les mouvements relatifs réguliers de la terre et du soleil, altérations dont on tient facilement compte, la chromosphère solaire est le siège de mouvements violents; d'autres causes mal connues peuvent agir, et l'on a des exemples de changements, momentanés il est vrai, mais extrêmement importants de ce spectre.[1] Si les nouvelles théories solaires de M. Julius[2] se confirmaient, de petites variations de longueurs d'onde

[1] THOLLON, *Annales de l'Observatoire de Nice;* HALE, ASTROPHYSICAL JOURNAL, **16,** 220, 1902.

[2] W. H. JULIUS, "Solar Phenomena, Considered in Connection with Anomalous Dispersion of Light," ASTROPHYSICAL JOURNAL, **12,** 185, 1900.

"Peculiarities and Changes of Fraunhofer Lines Interpreted as Consequences of Anomalous Dispersion of Sunlight in the Corona," *ibid.*, **18,** 50, 1903.

"Les théories solaires et la dispersion anomale," *Revue générale des Sciences,* **15,** 480, 1904.

des raies solaires n'auraient plus rien de surprenant. En outre comme nous l'avons fait déjà remarquer, le spectre solaire n'est jamais employé comme spectre de comparaison, pas plus par les astronomes que par les physiciens; on se sert toujours de radiations artificielles, qui ne peuvent être que des étalons secondaires si le spectre solaire est pris comme spectre fondamental. L'emploi de raies sombres pour la définition d'une longueur d'onde est peut-être moins simple et moins avantageux que l'emploi de raies brillantes. Enfin, la mesure de la longueur d'onde d'une raie sombre par les méthodes interférentielles, qui seront sans doute employées dorénavant pour les mesures fondamentales, est beaucoup moins facile et un peu moins précise que la mesure d'une raie brillante.

Si l'on se décide à abandonner le spectre solaire pour l'établissement de l'échelle fondamentale des longueurs d'onde, on est forcément amené à prendre des sources de lumière artificielles donnant des raies brillantes (gaz rendu lumineux par des procédés convenables).

Les radiations choisies doivent satisfaire aux deux conditions fondamentales suivantes:

1. Il faut se mettre à l'abri de toute variation possible de la longueur d'onde, et pour cela définir exactement toutes les circonstances qui définissent l'état du gaz et la manière dont il est rendu lumineux. Cela exige certainement, quelques précautions, et sur certains points de nouvelles études sont désirables; mais on peut affirmer dès maintenant qu'il est possible d'avoir une constance absolue de certaines longueurs d'onde. En tout cas l'emploi de sources artificielles présente sur l'emploi de la lumière solaire cet avantage immense que l'on peut expérimenter sur elles, tandis que sur la lumière solaire, on ne peut qu'observer.

2. Il faut que les raies brillantes employées comme étalons soient suffisamment fines, de manière à définir une longueur d'onde bien déterminée. Il faudra éviter de se servir de raies accompagnées de satellites, à moins qu'ils ne soient très faibles, de telle sorte que la longueur moyenne ne diffère pas de celle de la composante principale, ou qu'ils ne soient suffisamment écartés pour que l'on puisse employer l'une des composantes bien déterminée.

On peut dès à présent affirmer que les spectres de certains métaux dans l'arc électrique donneront un grand nombre de raies satisfaisant à toutes les conditions requises.

Les étalons primaires ainsi choisis pourraient ne pas être extrêmement nombreux; quelques dizaines dans le spectre visible et ultraviolet suffiraient probablement; il serait facile de leur rapporter par interpolation toutes les autres raies que l'on jugerait commode d'employer comme étalons secondaires. Quant au spectre solaire, son étude rentrerait dans le domaine de l'astronomie physique, comme un moyen extrêmement puissant pour l'étude des phénomènes solaires.

Quant à la question de détermination de la longueur d'onde de ces radiations étalons, elle semble *a priori* pouvoir être faite suivant une unité de longueur arbitraire; on pourrait songer à prendre par exemple la longueur d'onde de la raie rouge du cadmium égale à l'unité; le nombre caractérisant une longuer d'onde serait alors le rapport de cette longueur d'onde à la longueur d'onde du cadmium, mais il semble bien préférable d'adopter une unité rattachée directement au système métrique; les comparaisons des longueurs d'onde entre elles comportant, semble-t-il, des erreurs du même ordre que celles qui ont pu être commises dans la mesure de la longueur d'onde de la raie rouge du cadmium faite par MM. Michelson et Benoit; ces dernières erreurs, si elles existent, n'apparaîtraient pas; la mesure en valeur absolue d'une seule longueur d'onde suffit d'ailleurs puisque l'on peut, sans difficulté, par la méthode interférentielle, comparer la longueur d'onde d'une radiation quelconque à celle de la radiation choisie comme étalon primaire. C'est, du reste, ce que nous avons fait dans tous nos travaux sur ce sujet, et une longue pratique nous a montré que l'emploi des raies du cadmium ne présente pas de difficultés, et définit une longueur d'onde avec une précision qu'il sera bien facile de dépasser. Nous avons déjà donné les longueurs d'onde d'un certain nombre de raies du fer comparées de cette manière à la raie fondamentale du cadmium.[1] Tout récemment M. Kayser a préconisé la même solution, et a annoncé que des mesures de ce genre étaient en cours dans son laboratoire.[2]

M. Hartmann a proposé récemment[3] de choisir une unité de longueur telle que les nombres de Rowland soient le moins possible altérés; en d'autres termes, les rapports entre les nombres de Rowland

[1] *Ann. de Chim. et de Phys.*, Janvier 1902, et ASTROPHYSICAL JOURNAL, **15**, 73, 261, 1902.

[2] *Ibid.*, **19**, 157, 1904. [3] *Ibid.*, **18**, 167, 1903.

n'étant pas exacts, on ne peut dire qu'ils soient rapportés à une unité définie quelconque, mais on peut chercher une sorte d'unité moyenne, telle que ces nombres soient altérés le moins possible; M. Hartmann a calculé une table de correction, simple transformation numérique de la nôtre, qui est basée sur cette condition. Il est évident que si, comme nous le proposons, on se décidait à reconstruire de toutes pièces une nouvelle échelle de longueurs d'onde, cette solution n'aurait aucune raison d'être. D'ailleurs pour que l'énorme travail de Rowland ne soit pas perdu (on sait que la *preliminary table* contient environ 20,000 raies), il suffirait de comparer par interpolation quelques raies solaires avec les étalons fondamentaux, et de construire une table de corrections plus étendue et peut-être plus exacte que celle que nous avons donnée.

Influence de l'air.—Les longueurs d'onde doivent être définies par leurs valeurs dans l'air. Les valeurs absolues étant très notablement affectées par les variations de température et de pression, il convient, comme l'ont fait MM. Michelson et Benoit, de définir soigneusement ces conditions pour les mesures absolues; mais les rapports des longueurs d'onde sont très peu affectés par les variations atmosphériques, à cause de la faible dispersion de l'air. Cependant, dans des conditions extrêmes de température et de pression, et pour les extrémités du spectre, les variations des rapports des longueurs d'onde peuvent porter sur les millionièmes. Il y a donc lieu dans la définition de l'échelle des longueurs d'onde de dire à quelle température et à quelle pression elle a été établie, mais une définition assez grossière de ces conditions suffit. Dans les mesures par interpolation, il n'y a jamais à tenir compte des conditions atmosphériques.

La solution qui s'impose consiste donc à adopter comme étalon fondamental une raie du cadmium sous certaines conditions de température et de pression, produite dans des conditions rigoureusement déterminées, connue en valeur absolue, grâce aux mesures de MM. Michelson et Benoit.

Les conclusions du présent rapport sont donc:

I. Il y a lieu d'établir une échelle nouvelle de longueurs d'onde étalons.

II. Ces longueurs d'onde seront celles de radiations dues à des sources artificielles, parfaitement définies, et susceptibles d'être reproduites toujours les mêmes.

III. Elles seront mesurées par des expériences spéciales faites par différents expérimentateurs relativement à la radiation rouge du cadmium, produite par le passage d'un courant alternatif ou d'une décharge de bobine dans un tube de Michelson à électrodes d'aluminium dont le tube capillaire a 1 cm de longueur et 2 mm de diamètre.

IV. Provisoirement, et jusqu'après de nouvelles expériences, la longueur d'onde de la raie rouge du cadmium ainsi définie sera considérée comme égale à 6438.4722, dans l'air à 15° sous la pression de 760 mm de mercure.

V. Parallèlement à ce travail, on déterminera une courbe de corrections relatives aux mesures de Rowland.

Laboratoire d'Essai
du Conservatoire National des
Arts et Métiers, Paris.
Université de Marseille.

NEW STANDARDS OF WAVE-LENGTH.

By H. KAYSER.

FOR many years spectroscopists had to be content with the determinations made by Ångström for the visible part, and extended later by Cornu to the ultra-violet part as far as λ 3000. For the wave-lengths to λ 2200 Liveing and Dewar and Hartley gave the first measurements. At that time errors between 1 and 5 Ångström units were not infrequent, *i. e.*, wave-lengths could be determined to only about one one-thousandth of their value. An immense advance was made by Rowland. He no longer tried to determine absolutely the wave-lengths of a great number of lines by the grating, but, applying the much more accurate method of coincidences, found the values of some hundred lines between $\lambda\lambda$ 2000 and 7600, relative to a value of the D lines taken somewhat at random from the absolute measurements by Bell, Kurlbaum, Müller and Kempf, Peirce. The method of coincidence, as is well known, is founded on the theory of the concave grating, by which at the same point a line of wave-length λ in the first order, of $\frac{\lambda}{2}$ in the second order, of $\frac{\lambda}{3}$ in the third, and so on, is brought to focus.

Rowland expressed the opinion that none of his standards could deviate by a hundredth of an Ångström unit from the correct value, and this seemed probable when one considered the first-rate instruments used by Rowland, and the eminent ability of Rowland and his assistant Jewell for such work.

This faith received the first shock when Michelson by his beautiful interferometer method determined at Paris, in collaboration with Benoit, the absolute values of three *Cd* lines. Not only the absolute values were greatly different from Rowland's, but the relative values also showed discrepancies amounting to more than 0.03 Ångström units. Rowland's system received a much severer blow, when Professors Pérot and Fabry by another kind of interferometer measured many solar and iron lines in the visible spectrum and showed that Rowland's system contained values alternately too high and too low.

It is now quite certain that Rowland overestimated the correctness of his measurements, and that in using his numbers as standards we never can be sure of obtaining relatively exact numbers within 0.01 unit. Besides these fluctuating errors pointed out by Pérot and Fabry, many of Rowland's lines have individual errors amounting to 1 or 2 hundredths, as anyone may see by using different standards for the determination of the same line. Some years ago I tried to give a table of iron standards free from these individual errors, but of course my table, being based on Rowland's, is subject to nearly the same fluctuations of values.

For the, in practice, more important ultra-violet part of Rowland's system we have no evidence whatever of correctness or incorrectness; but it is highly probable that it will contain errors of the same kind as the visible part.

If one tries to determine the wave-length of a sharp line—and nearly 99 per cent. of all spectrum lines can be obtained sharp—by interpolation between neighboring standards, he will find it not difficult by using a good concave grating and a good measuring engine to get with a single measurement the value within five thousandths of a unit, if he uses the same standards; but using others he may get differences of one or two hundredths. So it appears that the uncertainty of our wave-length determinations is caused by the insufficiency of Rowland's standards.

To say a word on the importance of obtaining wave-lengths as correct as possible, for astronomical purposes, for the identification of elements, for the series relations, etc., seems needless. Thus the necessity of creating a new system of standards is evident, and it seems time to consider the way by which a better system may be produced and the errors committed by Rowland avoided.

One of Rowland's errors was the use of mixed solar and arc lines, which he thought were of identical wave-length. Since that time we know, by the publications of Jewell, Humphreys, and Mohler, that they are different. This cause of error could thus be easily avoided, and it seemed as if by applying the method of coincidences to arc lines only, a correct system could be produced. During the last twelve years I have three times tried to do so, but have always failed—I did not know why.

After the publication of Messrs. Pérot and Fabry's measurements of iron lines I formed the plan of founding the method of coincidences on their measurements; it is much easier to get a system of standards throughout the spectrum by using many fundamental lines than by using a single one. But as I was not sure of the absolute correctness to Pérot and Fabry's numbers, though they seemed very reliable, I felt obliged to convince myself. This can be done by the method of coincidences, as one line can be got by that method from others.

While engaged in this research there appeared a paper from Michelson showing that errors in the ruling of gratings are possible which make the method inapplicable: a line of given wave-length in the first order does not coincide with another line of half that wave-length in the second order. I immediately changed the scope of my investigations and made some tests to see if Rowland's gratings have such an error. I have two of Rowland's largest gratings ruled on his second and third dividing engines. The comparison of results obtained with them by the method of coincidences gave differences of more than 0.03 Ångström units, while repeated determinations with each grating alone agreed to a few thousandths.

It is thus apparent that Rowland's gratings are not suited for the application of this method, and it is probable that the larger part of the errors in Rowland's system are due to this cause. As gratings are therefore not to be used for exact absolute determinations, they are useless also for relative measurements, if great accuracy is desired. At present we have no better engines, though different American physicists are planning new constructions, as Michelson and Wadsworth. Time must show if their engines are better. We are now under the deplorable necessity of disregarding the method of coincidences in the creation of a new system of standards; the spectra of gratings can be used only for interpolation between standards not too far apart.

It seems to me, then, that the only way open consists in the determination of a great number of arc lines with some interferometer, and probably Pérot's and Fabry form of that instrument will prove most appropriate. Its use is not easy; it requires long practice even for the visible part of the spectrum, and for the ultra-violet part the difficulties will be much greater. Not a single observer, but several,

should undertake this task, and when their numbers agree to one or two thousandths of a unit for, say, fifty or a hundred lines throughout the spectrum, then the grating may again be applied for getting by interpolation some hundred lines more. Then it will be possible to get all the spectral lines accurate to some thousandths of a unit, a great advance will have been made, and many questions that are now inapproachable can be answered.

Of course, all the measurements of spectra already made—and I have myself worked a good deal in that direction—will be useless as far as wave-lengths are concerned. Professor Hartmann, of Potsdam, thinks it already now possible to calculate from Pérot and Fabry's measurements a table of corrections, by the application of which Rowland's measurements, and all the others based on Rowland's standards, may be corrected. I think that is impossible. We do not even know that the fluctuations in Rowland's system are truly known, as very few lines have been remeasured by Pérot and Fabry, and they may have fallen on some lines with large individual errors, and the curve of corrections may become quite another by the use of other lines. But even if that were not the case, who knows from what individual standards every line of a spectrum has been determined? Hasselberg is the only man who gives in his tables the standards between which he has interpolated, so that his numbers can be corrected when corrections to Rowland's standards are known. The greater errors of individual lines in Rowland's tables cannot be corrected. And, further, it is quite impossible to deduce any table of corrections for the ultra-violet part. As the method of coincidences has broken down for gratings which have been tested, we can get no conclusion whatever as to the behavior of Rowland's system in this part of the spectrum without new absolute measurements.

I therefore think Rowland's system must be entirely abandoned and a new system created. Since for all spectroscopic purposes only relatively correct values are needed, the new system may be based on Rowland's incorrect value for the D lines; then new measurements would coincide within some hundredths instead of tenths of a unit with the old ones. I think it more logical, if an entirely new system is to be created, to base it on Michelson's absolutely exact values of the *Cd* lines. But this question is of minor importance.

SOME TOTAL SOLAR ECLIPSE PROBLEMS.[1]

By C. D. PERRINE.

REAL progress in eclipse work dates from the applications of photography and spectroscopy to the subject. Indeed, the fundamental question whether the corona belongs to the Sun or is a lunar effect was first definitely answered by the sensitive plate.

These methods of investigation, coupled with a fuller realization that the corona is a part of the Sun, and that every fact concerning our Sun is of the utmost importance in the study of the stars in general, brought eclipse problems into great prominence. Following rapidly came the important discoveries of the bright lines in the corona and chromosphere, of the reversing-layer, and of variations in the general form of the corona with the Sun-spot period. Later came the detection of a few Fraunhofer lines in the corona, of the irregular distribution of the "coronium" stratum, of interesting details in the coronal structure, particularly the "hoods" over the prominences, and of the low effective temperature of the corona as measured with the bolometer.

Observations of the last eclipse furnished strong evidence that the corona, Sun-spots, and prominences are closely related phenomena, and that the light of the outer corona is almost wholly reflected or diffracted photospheric light, whereas that of the inner corona is chiefly inherent.

At the 1901 eclipse an unusually favorable opportunity presented itself for determining the motion of coronal matter in a region of abnormal disturbance, from a comparison of photographs secured at different stations. Unfortunately, suitable observations for this purpose were available over only a small interval of time, and the most we can say is that the velocities at right angles to the line of sight were certainly less than 20 miles (32 km) per second. This result points to comparatively low velocities as the normal condition in the corona.

[1] Read at the International Congress of Arts and Science, St. Louis, September 23, 1904.

The question of the existence of intra-Mercurial planets is a distinct eclipse problem. Observations at the last eclipse indicate that there is no such planet as bright as the fifth magnitude, nor any great number as bright as the eighth magnitude.

This, briefly, is our knowledge of eclipse phenomena at the present time—a knowledge which is seen to be general, rather than detailed. The work already accomplished has been of the nature of a reconnaissance, and suggests a concentration on details as the line of future development.

The problem which promises to be solved first, and to be eliminated from eclipse programs, is that of the existence of intra-Mercurial planets. At present there seems little probability that bodies of appreciable size exist and the eclipse of 1905 ought to settle the question so far as objects brighter than the tenth photographic magnitude are concerned. This appears to be a sufficiently faint limit for the present, as a body of tenth magnitude in this region could scarcely be over 12 or 15 miles in diameter. Approximately a million such bodies would be required to explain the observed anomalies in the motion of Mercury. The long-focus cameras used at the last two eclipses proved very efficient in this search. Under good conditions they can record objects of the tenth photographic magnitude, and their power could be readily increased if necessary.

In the event that no planet is found, it will remain for the mathematical astronomer to discover the cause of the discrepancies in Mercury's motion. However, it is not impossible that a ring of very small bodies, numbering many millions, perhaps beyond the power of practicable eclipse instruments, may exist within the orbit of Mercury.

The structure of the corona is one of the most important of eclipse problems and is best studied with long-focus cameras. These should give as large an image as the atmospheric conditions will permit. Cameras of forty feet focus are readily pointed directly at the Sun (a method which has, I think, some advantages), and it is probable that those of 50 or even 60 feet might be so used. Instruments of greater focal length would be advantageous under good atmospheric conditions, but it would be advisable to mount them horizontally.

The entire change in the appearance of the detailed coronal feat-

ures from one eclipse to another shows clearly that the coronal matter is in motion. The appearance of the streamers, and their evident connection in so many instances with prominences and other eruptive phenomena on the Sun's surface, strongly indicate motions away from the Sun. Radiation pressure must also be concerned to a great extent in the outward transportation of the very minute particles of matter. On the other hand, the forms of the hoods and the low order of velocities of coronal matter (indicated by observations at the 1901 eclipse) leave little room for doubt that there is motion inward as well as outward.

It is impossible to overestimate the value of a knowledge of these motions, particularly if motions *in* the line of sight, as well as at *right angles* to it, can be secured. Accurate measures of these motions at several points in the path of a streamer, for example, would permit a satisfactory determination of its form and point of origin to be made. The motions at right angles to the line of sight should be determinable from photographs obtained at intervals of from one to two hours. The 1905 eclipse offers especially favorable opportunities in this respect. Stations may be occupied from Labrador to Egypt. Between these extreme stations there is a difference of two and one-half hours in the times of totality.

The success at the last eclipse in recording the Fraunhofer lines out to a distance of 45′ from the solar limb, through clouds which allowed only a small portion of the light to pass, indicates a possibility of securing some line-of-sight results from the photospheric light in the corona. However, more powerful spectrographs and plates of greater sensitiveness than those heretofore applied would have to be used. Difficulties would arise in the interpretation of such results, for the motion of reflecting and diffracting matter at *right angles* to the line of sight would cause a displacement of the lines as well as the motion *in* that line; but the value of the former component would perhaps be available from the comparison of large scale coronal photographs.

The general character of the light of the corona seems to be pretty conclusively determined. A very small proportion has a bright-line spectrum, which indicates a gaseous nature. The substance emitting the bright-line radiations is thus far unidentified with any

terrestrial elements. The greater part of the light from the inner corona gives a spectrum without any visible trace of the Fraunhofer lines. It is undoubtedly due to incandescence of solid or liquid matter. Practically all of the light of the outer corona gives a spectrum which is an exact counterpart of the solar spectrum. Polarization results are in full accord with this view.

The only evidence which appeared to be at variance with the theory of an incandescent inner corona is the bolometric observation of the Smithsonian party at the 1900 eclipse. This observation showed a small quantity of heat, but very much less than had been anticipated, on the theory of incandescence. The effective coronal temperature observed was but a few degrees higher than the temperature of the observing room. Arrhenius has recently shown that the heat radiation observed at that eclipse is as much as could be expected from incandescent matter under the conditions probably prevailing in the corona. The close agreement of results from such widely differing methods gives considerable confidence in the theory of incandescence.

Theoretically, it is possible, by means of a well-known law, to determine the temperature of the corona from the position of the maximum of intensity in its continuous spectrum. Such a study would be most important, but it may be doubted whether the necessary observations can be secured, from the fact that for the temperatures probably existing in the corona the maxima would fall in the extended region of the spectrum between the green and the infra-red. It would be difficult to observe this region of the spectrum either visually or photographically.

Schuster has shown that with sufficiently accurate polariscopic observations it would be possible to determine the law of distribution of coronal matter, and whether the materials are moving away from, or toward, the Sun. When we consider, however, the variable conditions which are undoubtedly present along almost any line of sight through the corona, it seems practically impossible to obtain trustworthy results except perhaps in the isolated polar streamers of "minimum" coronas.

Photometric determinations of the brightness of the corona, as a whole and of the different features, if systematically carried out, furnish an excellent test for solar and coronal theories. It is still

uncertain whether the "maximum" or "minimum" coronas are brightest. Photometric observations of the coronas of 1893, 1898, and 1900 by the English expeditions indicate a decided increase in brightness [in the ratios 3, 6, and 8 respectively],[1] whereas the large-scale photographs made by the Lick Observatory expeditions upon a systematic plan show a *decrease* for the same eclipses [in the ratios 10, 8, and 5]. [In obtaining the results from the large-scale photographs, the accidental variations due to development, etc., have been eliminated as far as possible by a combination of the results from a number of negatives. The plates used in 1893 were of a lower sensitometer number than those of 1898 and 1900. It seems reasonable to suppose that the later plates are in general more rapid than the earlier ones, the effect of which would be still further to accentuate the decrease in observed brightness. Again, the large-scale photographs of 1893 were taken with the Sun at a lower altitude than at the eclipses of 1898 and 1900; and a correction for this fact would further favor the greater brightness of the 1893 corona.] The desirability of carrying a well-considered program of photometric observations through one or more solar periods is evident.

The wave-lengths of the lines in the spectra of the chromosphere, reversing-layer, and corona should be determined with the greatest possible accuracy in order to make their identification with terrestrial elements more certain. Professor Liveing inclines strongly to the belief that the volatile gases recently discovered in the Earth's atmosphere—argon, krypton, neon, and xenon—are present in the outer strata of the Sun, as observed at eclipses. The decision of this question awaits more accurate determinations of the lines in the flash spectrum.

The use of a moving plate to furnish a continuous record of the spectrum of the Sun's edge is of especial value in obtaining a knowledge of the heights of the substances and of possible changes in the wave-lengths of the lines in their spectra, as their strata are covered and uncovered by the advancing Moon. The fixed plate also has its advantages, in that familiar lines are recorded, but the result is an integrated effect, and changes during the exposure are lost.

A careful study of the depths and distribution of the gaseous

[1] Bracketed portions were not read.

constituents of the corona throughout one or more Sun-spot cycles can scarcely fail to disclose very important facts. The variation in the distribution of "coronium" as observed at different eclipses is an indication which should be followed up closely.

Although the corona is an appendage of the Sun, to what extent it partakes of his surface rotation is questionable. It would, therefore, be of great value to have a determination of rotation for both the gaseous and the non-gaseous portions, from line-of-sight observations. But we must not overlook the difficulties in the way of the solution of this problem. A *difference* in the coronal velocities east and west of the Sun might be detected with moderate dispersive powers such as it may be possible to employ in the not remote future. The determination of the *law* of rotation would require very much more accurate data, however.

The most pressing problem in connection with eclipse work is that of finding a method for observing the corona in full sunshine, thus permitting us to secure a continuous record, as in the case of the prominences. All the methods thus far tried have failed to give positive results.

So far as the gaseous constituent of the corona is concerned, although its quantity is relatively very small, it is theoretically possible to obtain images in its bright lines on the assumption that they are monochromatic. The non-gaseous corona presents much greater difficulties because it is impossible to isolate its radiations by any simple spectrographic method. The attempt to observe the corona in full sunshine by means of the spectrograph using a double-slit gave promise of success, theoretically, which has not yet been realized. It may be pointed out that this method of diffusing the atmospheric glare by taking photographs of the strictly continuous light of the corona through an atmospheric (solar) dark line could only be expected to give an image of the inner corona where the light due to incandescence predominates. Results obtained at the last eclipse indicate that this would not be more than 10 minutes from the limb. The outer corona in which photospheric light predominates would not impress itself through a dark line.

The extremely small quantity of heat radiated by the corona seems to preclude the hope of observing it by temperature methods.

The properties of polarized light enable the contrast between the strongly polarized portions of the corona, and the unpolarized sky illumination to be greatly increased. Experiments at Mount Hamilton, in which the theoretical contrast between corona and sky was increased twelvefold by polariscopic methods, did not reveal the corona to the eye.

There exists at every eclipse the desirability, and at many eclipses the imperative need, of immediate confirmation by different observers. These considerations prompt the thought that too many expeditions with well-considered plans of work cannot engage in eclipse observations, and that an interchange of ideas and discussion of plans in advance is highly desirable. For those problems which require observations extending over many eclipses, or over a number of spot-cycles, the advantages of co-operation are especially great.

LICK OBSERVATORY,
September 1904.

ON A NEW METHOD FOR THE MEASUREMENT OF STELLAR SPECTRA.[1]

By J. Hartmann.

When Vogel in 1888 introduced the photographic method for determining the radial velocity of stars, he used two coincidence methods in measuring the spectrograms. In his "first method" there is laid upon the plate to be measured a plate of the solar spectrum taken with the same spectrograph. Then the positions of several lines in the stellar spectrum are measured with respect to the corresponding lines in the solar spectrum, and thereupon the displacement of the artificial comparison lines as compared with the corresponding lines of the solar spectrum. The difference of the two displacements then yields the displacement of the stellar spectrum with respect to the terrestrial comparison spectrum. In Vogel's "second method" this latter displacement was measured directly.

The reduction of the measurements is exceedingly simple in the case of this procedure of utilizing coincidences. The desired velocity of the star in kilometers is obtained by simply multiplying the displacement measured with the micrometer screw by a factor which is nearly constant for each wave-length. It was a further great advantage of Vogel's "first method" that the relative position of the stellar and solar spectra was very sharply determined by the use of a very considerable number of lines. The advantage hereby gained was, however, in part lost again by the fact that the displacement of the superposed solar spectrum in respect to the artificial comparison spectrum could be determined by measurements of but a single line. This disadvantage of all coincidence measurements—that they are always restricted to the few lines which are simultaneously present in the comparison spectrum and in the stellar spectrum—appears still more directly in Vogel's "second method." In this process the further assumption is made that the lines in question have precisely the same wave length in the stellar and in the comparison spectrum—a postulate which certainly is not rigorously fulfilled.

[1] Presented to the International Congress of Arts and Science, St. Louis, September 1904.

It is, therefore, now customary to employ the coincidence process only where a quick and easy determination of provisional results is wanted. For the definitive treatment of the spectrum plates a method is now generally employed which permits the measurement of all lines desired of the stellar spectrum. The reduction of such measurements is then effected by the aid of a dispersion formula which establishes a relation between the wave-length of a line and the reading of the micrometer screw with which the spectrum was measured. I published in the *Astronomische Nachrichten* (**155**, 81–118, 1901) a complete description of this rigorous procedure.

This generally employed reduction process has, however, several quite serious disadvantages, of which I will cite only the two following as of the most consequence. First, the measurement as well as the computation, even in the simplest form that I have given, is still so laborious that it is quite impossible fully to exhaust each spectrum of the class having numerous lines—that is, to measure *all* of the lines by this method. The consequence is that hitherto the observers have always limited themselves to the measurement of a few lines, thus sacrificing the complete utilization of the rich material contained on the plates. It is only in case of spectra of the first type, having few lines, that it has been possible to utilize all of the lines for determining velocity. This has been repeatedly done, so that the measurement of the spectrograms of this sort may be regarded as definitively accomplished.

The second defect in the procedure in question is that the wave-lengths of the lines measured in the comparison spectrum must be assumed to be accurately known. Inasmuch as a change of wave-length of from 0.01 to 0.02 tenth-meters corresponds to a velocity of 1 kilometer, the wave-lengths of all the lines measured would have to be accurately known, at least to 0.01 tenth-meter, if the fractions of a kilometer of the velocity are to be guaranteed. But wave-lengths as accurate as this are available only in isolated instances. Just recently I have pointed out how important it is for astrophysics to have measurements of the wave-lengths carried out referred to a uniform system. Measurements of velocity cannot be definitively reduced so long as these wave-length determinations have not been completed; but still many years will elapse before sufficiently sharp

wave-lengths of all the elements observed in stellar spectra will be available.

But even when these wave-lengths of terrestrial substances are accurately determined, there will still remain the assumption, which surely is only approximately correct, that the lines in question also have precisely the same wave-length in the absorption spectrum of the stars observed.

In order to overcome all these difficulties, and to enable an entirely definitive determination of velocity to be made at present, for all many-lined spectra, I developed several years ago a method which depends upon a wholly new principle of measurement. While in all previous methods the measurement was effected by setting a single thread successively upon the lines of the stellar spectrum and those of the comparison spectrum, the new procedure depends upon the *simultaneous setting of numerous lines of one spectrum on the lines of another spectrum.*

The stellar spectrum is photographed as heretofore, and therefore has on both sides the lines of a terrestrial comparison spectrum. With the same spectrograph a solar spectrum is now photographed in a similar manner and with the same comparison spectrum. The two plates are placed in a specially constructed measuring microscope, and the observer then sees in the field of view, close to and on both sides of the stellar spectrum, the corresponding region of the solar spectrum; and similarly, alongside of the comparison spectrum of the star, that of the Sun. It is then only necessary to move the solar spectrum by the screw so that the lines of the stellar spectrum coincide with those of the solar spectrum, and then so that the lines of the two comparison spectra coincide. The difference of the settings then gives directly the displacement sought.

It is seen that this process requires neither an accurate knowledge of the wave-lengths of the lines used nor any extensive computation of any sort. In fact, it is no longer necessary for the observer to make the settings upon each individual line, for the apparatus is so constructed that long stretches of the adjacent spectra, which in part may consist of irresolvable groups of lines, may always be brought simultaneously into coincidence. The observer is thus enabled to employ *all* the lines of many-lined spectra for measurement; the

measurement is correspondingly more precise, but is, nevertheless, very rapidly executed.

In the reductions, which are as simple as in the case of Vogel's coincidence methods, only the single assumption is made that the lines have the same wave-lengths in the spectra of star and Sun. This assumption is undoubtedly the only permissible one for all stars of the second type, for the treatment of which the process now described was primarily designed. At least this assumption is probably nearer the facts than that hitherto always made, namely, that the lines of these stellar spectra have precisely the same wave-lengths as those in terrestrial substances investigated in the laboratory.

A more precise description of the apparatus for measurement, as well as of the procedure in observing, will be given later.

ASTROPHYSICAL OBSERVATORY, POTSDAM
August 2, 1904.

A DESIDERATUM IN SPECTROLOGY.[1]

By Edwin B. Frost.

It is to be presumed that most astronomers will agree in regarding the knowledge of the stellar evolution as one of the greatest ultimate problems of astrophysics. It can hardly be questioned that great simplification would be gained, and the co-ordination of observations be rendered more certain, if a comprehensive scheme of stellar classification according to spectra could be adopted by all workers in this field. It has seemed to me that the occasion of an International Scientific Congress is particularly appropriate for briefly considering whether a beginning should not soon be made toward developing such a system by the combined efforts of the workers in this department of astrophysics. It is obvious that the common consent of the parties interested is essential to the successful erection of a structure of this sort, and it cannot be the work of any individual, or of any group of individuals which does not fully and widely represent those concerned.

If someone should fear that this is a proposal for the construction of an artificial system, which would only add to the nomenclature and terminology of astrophysics, let us consider for a moment the present status of the question of stellar classification. Differences in stellar spectra were recognized by Fraunhofer. Many years later Rutherfurd published a brief grouping of them. But the classification proposed by Secchi in 1866 has been most widely used, doubtless by reason of the simplicity and obvious distinctness of its four types. Based upon visual observations alone, it must necessarily lack comprehensiveness, and this has led to the suggestion by E. C. Pickering[2] of the addition of a fifth type to include stars showing bright lines.

Vogel's system, logically developed along the line of his pioneer and prescient views of stellar evolution, has been of great service for thirty years. With the increased knowledge acquired from more

[1] Paper read before the Section of Astrophysics of the International Congress of Arts and Science, St. Louis, September 1904.

[2] *A. N.*, 127, 1, 1891.

extensive and better observational data (in short, from the introduction of spectrography, or the application of photography to the spectroscope), a restatement of the distinguishing features has recently been made, happily by the eminent founder of the system. It now includes ten subdivisions.

In connection with the extensive operations in the field of stellar spectroscopy at Harvard Observatory, a more extended system has been found necessary by Professor Pickering for recording the observed facts, and, in a sense, it may be said that three different classifications have been used in the Harvard publications. The development was carried farthest in the scheme of Miss Maury, which includes more than seventy subdivisions or shadings. Other groupings have been proposed with considerable elaborateness, and as the result of careful study of large quantities of observed data, by Sir Norman Lockyer, and also by Mr. F. McClean. These newer systems have not been proposed by their authors from a desire to associate new classifications with their researches, but from their feelings of the insufficiency of existing systems, or of the lack of adaptability of the existing systems to the purposes of their own work. The systems have all been of service, but the very existence of so many shows quite clearly the need of combined action tending toward the adoption of one of them, or toward their supersession by a new system formed by international co-operation, and representing, as far as possible, the combined views of astrophysicists.

As an illustration of the confusion and mnemonic difficulties of the present classifications,[1] which (excepting Lockyer's of 1899) are merely distinguished by numerals or by letters, let me cite the case of the star *Procyon*. It is assigned by the different authorities as follows:

[1] Literature: SECCHI, *Comptes Rendus*, **63**, 626, 1866; H. C. VOGEL, *Astronomische Nachrichten*, **84**, 113, 1874; *Sitzungsberichte der k. Akad. zu Berlin*, 1895, 947–958; ASTROPHYSICAL JOURNAL, **2**, 333–346, 1895; *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, **12**, 6–8, 1899; E. C. PICKERING, *Annals of Harvard College Observatory*, **27**, 1890; MISS A. C. MAURY, *ibid.*, **28**, Part I, 1897; MISS A. J. CANNON, *ibid.*, **28**, Part II, 1901; J. N. LOCKYER, *Phil. Trans.*, **184**, 724, 1893; *Proc. R. S.*, **65**, 186, 1899; "Catalogue of 470 of the Brighter Stars Classified According to Their Chemistry at the Solar Physics Observatory, South Kensington," 1902; F. MCCLEAN, "Comparative Photographic Spectra of Stars to the 3½ Magnitude," *Phil. Trans.*, **191**, 127, 1898; "Spectra of Southern Stars," London, 1898.

Secchi, II; Vogel (1875), Ia; Pickering (Draper Catalogue), F; Lockyer (1893), A (β); Vogel (1895), Ia3; Miss Maury, XIIa; McClean Division III; Miss Cannon, F5G; Lockyer (1899), Procyonian. I venture to say that in this company there are not many who could instantly localize a spectrum of Vogel's Class Ia2; or of the Draper Catalogue E; or Miss Maury's VIIc; or of Lockyer's Taurian or Achernian groups.

The lack of defining power of the present numerical or literal groups seems very obvious. It will, perhaps, be felt quite as much by workers in stellar spectrography as by teachers, who cannot fail to keenly realize the student's difficulty in visualizing the spectra described as of "Type 3, Group VI" or Class "M." There is a great advantage in the use, in the classification of a kindred science, of such a term as "carnivora," which is available for all languages and is at once comprehensible.

The field is almost entirely open and untouched, as compared with other branches of science. It is not even agreed—indeed, has it been often considered?—whether there shall be used, in the comparative study of spectra, divisions corresponding to orders, families, and genera. Of the helpfulness of such divisions in other sciences there can be no doubt. Starting thus with the benefit of the experience of the students who use classification in other subjects, it should be possible in this subject to avoid the inconveniences, and even absurdities, which have attached themselves to systems in other branches of science, while utilizing their good points. It is, of course, an essential feature that only such terms should be used as will pass without any considerable change in all of the four principal modern languages.

In petrography, as a result of labors extending over a decade, by five or more American authorities in that field, a scheme of classification has recently been published which is commanding general attention among geologists. Had it been an international undertaking, and more widely representative, it would presumably find very prompt and widespread adoption.

In view of what has been said, it would be quite inappropriate for me, or perhaps for any individual, to advance at this time proposals for the new system. I would merely point out certain questions that

would arise in determining the bases of a classification. It is a distinction of the classifications of Vogel and of Lockyer that they logically correspond to the theories or hypotheses of their authors. They, accordingly, are not in marked agreement; and in the case of any system based upon theories of development, new theories, or an inversion of old theories, would necessarily carry with them still other new classifications. Is it not, therefore, desirable that any new system of classification should be based, rather, purely upon observed data? Theories of today may be reversed tomorrow, and, indeed, what we regard as observed facts may soon receive a diametrically opposite interpretation; nevertheless, no age has a surer basis for its reasoning than what it regards as its observed facts.

In any new classification, simplicity should, of course, be sought, and the subject naturally furnishes obvious distinctions; such are given by bright lines and dark lines, bright and dark bands, spectra having few lines and spectra having many lines, narrow lines and broad lines, bands of different aspects. Then, too, the classification according to chemistry is most natural, and probably reliable. The term "helium" class of spectra would seem unambiguous and definite, although, of course, we realize the presence of helium in the Sun, hence in the stars of the solar type. It is certain that the absence of the lines characteristic of a particular spectrum constitutes no proof of the absence of that element from the celestial body.

In the light of our present knowledge—or, better, in the darkness of our present ignorance—the use of temperature as a basis of classification would seem very doubtful; but, without committal to any theory, the different electrical behavior of the different lines—occurrence of enhanced lines, so called—might properly serve as a criterion in arranging some subdivisions.

It would not seem too much to expect that a new international classification should include at least one hundred separate subdivisions, for of the reality of the distinction between existing groupings there cannot be the slightest question in the minds of those dealing directly with spectrograms. But any new classification should be based upon the broadest facts and the most comprehensive data, already available and to be collected for the purpose in hand. Distinctions must not be based upon comparisons of merely the visual part of the spec-

trum, or of the narrow range in the blue and violet often studied; the whole extent of the ultra-violet attainable should also be included, and studies of the distribution of energy in the entire range of the spectrum should add important data.

In view of the necessary time involved, probably five years, for the fairly complete development of a new system of classification, and in view of the desirability of having the participation of those distinguished pioneers in stellar spectrographic work, yet happily with us, I submit the question: Is it not time that a beginning be made by the organization of an international committee to consider the question of a new classification of stellar spectra, representative of the observable facts of the first decade of the twentieth century?

YERKES OBSERVATORY,
September 15, 1904.

AN ELECTRIC THERMOSTAT.

By Horace Darwin.

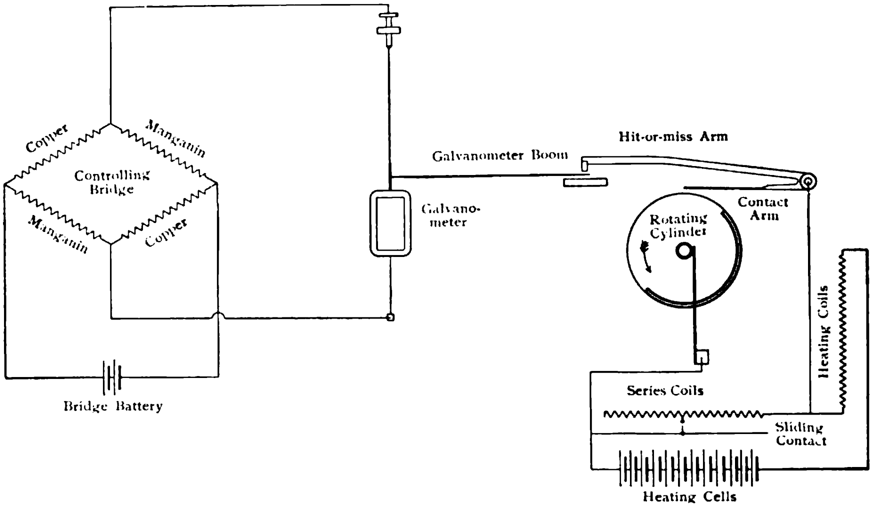
This thermostat was designed and constructed by the Cambridge Scientific Instrument Company, Ltd., as an adjunct to the spectrograph of the twenty-four-inch refractor of the Royal Observatory, Cape of Good Hope. The special object in view is to maintain the prisms and other parts as accurately as possible at a known constant temperature, over a considerable period; but, as the same devices are capable of a far more extended application, it appeared that an account of them might be of interest. The prisms are inclosed in a metal box which is surrounded by an outer chamber; the temperature of the air in this chamber is kept constant. The air is rapidly stirred by an electrically driven fan, and the temperature in the box varies very slightly, this slight variation taking place very slowly.

The same device has been used by Lord Berkeley in some delicate experiments he is trying. In this case it is an oil bath which is to be kept at a constant temperature. The oil is rapidly stirred by rotating fans made in the form of screw propellers, and the temperature did not vary by so much as $0^\circ.01$ C. for a week at a time.

The heat is supplied by electrically heated coils, and the regulation of the temperature is automatically effected by varying the current passing through these coils. Four resistances, two of copper and two of manganin, are arranged as a Wheatstone bridge, and are placed in the chamber or vessel the temperature of which should not vary. Through this controlling bridge a small and constant current is always passing. One pair of opposite arms are of copper, and the other pair are of manganin, and their resistances are such that they balance at the required temperature, and then no current passes through the galvanometer connected to the bridge. But as the temperature coefficient of the copper arms is large and that of the manganin small, a very small variation of the temperature will throw the galvanometer over to one side or the other. It is this movement of the suspended coil of the galvanometer which regulates the supply of current to the heating coils. The resistances of the arms of the bridge can

be altered at pleasure; this gives a means of altering the temperature at which the galvanometer balances, and consequently the temperature of the chamber can be adjusted to whatever is desired.

A current is always passing through these heating coils and through a set of resistances in series with them; we will call these the "series coils." The less the resistance of the series coils, the greater is the current which passes through the heating coils; and if the series coils are short-circuited, the effect of their resistance is eliminated.



If the galvanometer swings over to the cold side, this short-circuiting takes place by the closing of a key, and more heat is supplied to the heating coils. This key is closed intermittently and remains closed for a short time. The amount of heat supplied to the heating coils can be increased in three ways:

1. The key can be closed more frequently.
2. The length of time during which it is closed can be increased.
3. The resistance of the series coils can be reduced.

The movement of the galvanometer boom regulates the supply of heat in all three ways. The first effect of a fall of temperature is that the short-circuit key is closed more frequently; this more frequent action of the key automatically increases the duration of each successive closure, and this again automatically diminishes the resistance of the series coils.

We will now describe the mechanism by which this is effected.

The suspended coil of the galvanometer carries a light horizontal boom of considerable length. A motor continually drives a horizontal axis to which is fixed a cam; this cam lifts up an arm (which we will call the hit-or-miss arm) capable of rotating about a horizontal axis, and then allows it to fall again. As the hit-or-miss arm falls, its end passes through an opening in a plate over which the galvanometer boom can pass; if the boom is on the "too hot" side, the end of the arm on its way down hits it, presses it down till it comes in contact with the plate, and thus the arm is prevented from being lowered to its full extent; if, on the other hand, the boom is on the "too cold" side, the arm is free to pass through the opening. In this case the arm is lowered still farther and carries with it the contact brush which acts as a key and completes contact, short-circuiting the series resistances. This, as before stated, increases the heat given out by the heating coils and raises the temperature of the chamber or vessel. The contact brush in its lowest position presses against a rotating cylinder; this gives a good rubbing contact, and as part of the cylinder is cut away, the brush falls quickly from its edge and the sparking is reduced. This cylinder rotates on the same axis as the cam and is fixed to it, and is thus driven by the motor. The cam is driven at a rate of about one turn per minute; at each rotation the arm is lowered, and the position of the galvanometer boom determines whether the contact brush makes contact or not. When the arm is in its upper position, the galvanometer is perfectly free to take up its position of equilibrium. The galvanometer is, in fact, a very delicate rèlay. There are stops which prevent the boom from moving too far in either direction; if the boom is pressing against one of these stops, there is always danger of its sticking there. This is a well-known difficulty, and when it happens, the galvanometer is not free to take up its position of equilibrium. To overcome this difficulty, the end of the hit-or-miss arm is made of such a shape that both in the case it hits, or in the case it misses, the galvanometer boom is moved away from the stops.

The cylindrical surface on which the contact brush rests is also capable of movement along the axis about which it rotates. The cylinder is cut away in such a shape that at one end of this endwise movement, the contact made by the brush is of very short duration,

and when at the other end the contact is comparatively long, and in intermediate positions the duration is intermediate between these extremes. It is this endwise movement which gives the second method of regulating the duration of the contacts, and consequently of the heat supplied to the heating coils. Every time the contact brush reaches its lowest position and contact is made, the cylinder is moved a small amount endwise, and the direction of this motion is such that the next contact will be of longer duration. Suppose the chamber is of too low a temperature, then contact will be made at every rotation of the cam, and the contact will get longer and longer each time, till at last too much heat will be entering the chamber, when the hit-or-miss arm is prevented from reaching its lowest position and no extra heat enters the chamber at that rotation of the cam. The converse then takes place; every time contact is not made the cylinder is moved endwise so that the duration of the contacts is successively reduced. It follows from this that if the hit-or-miss arm hits and misses the galvanometer boom alternately, the cylinder has no endwise displacement given to it in the long run; but if the hits and misses are not equal, endwise displacement will occur, and this goes on lengthening or shortening the duration of the contacts until there are as many hits as misses in a given interval of time. This endwise displacement is given by the same cam which moves the hit-or-miss arm up and down; the cam is simple in its action, but difficult to describe.

The third method of regulating the supply of heat is by altering the amount of the resistance in the series coils. This is done automatically by the endwise movement of the cylinder, which throws in or cuts out some of the series coil resistances.

In many cases this last arrangement will not be necessary, but in the spectroscope at the Cape of Good Hope the external temperature varied greatly, and consequently the quantity of heat to be supplied had also to vary greatly. The prisms might be only 5° F. hotter than the external temperature at midday, and they might be as much as 40° F. hotter than the external temperature at night. Thus at night eight times as much heat has to be supplied as during the day in order to keep the chamber at a constant temperature. Mr. Lunt, of the Cape of Good Hope Observatory, suggested this arrangement.

CAMBRIDGE, ENG.

THE ATLAS CHART FOR *T ORIONIS* EXTENDED.

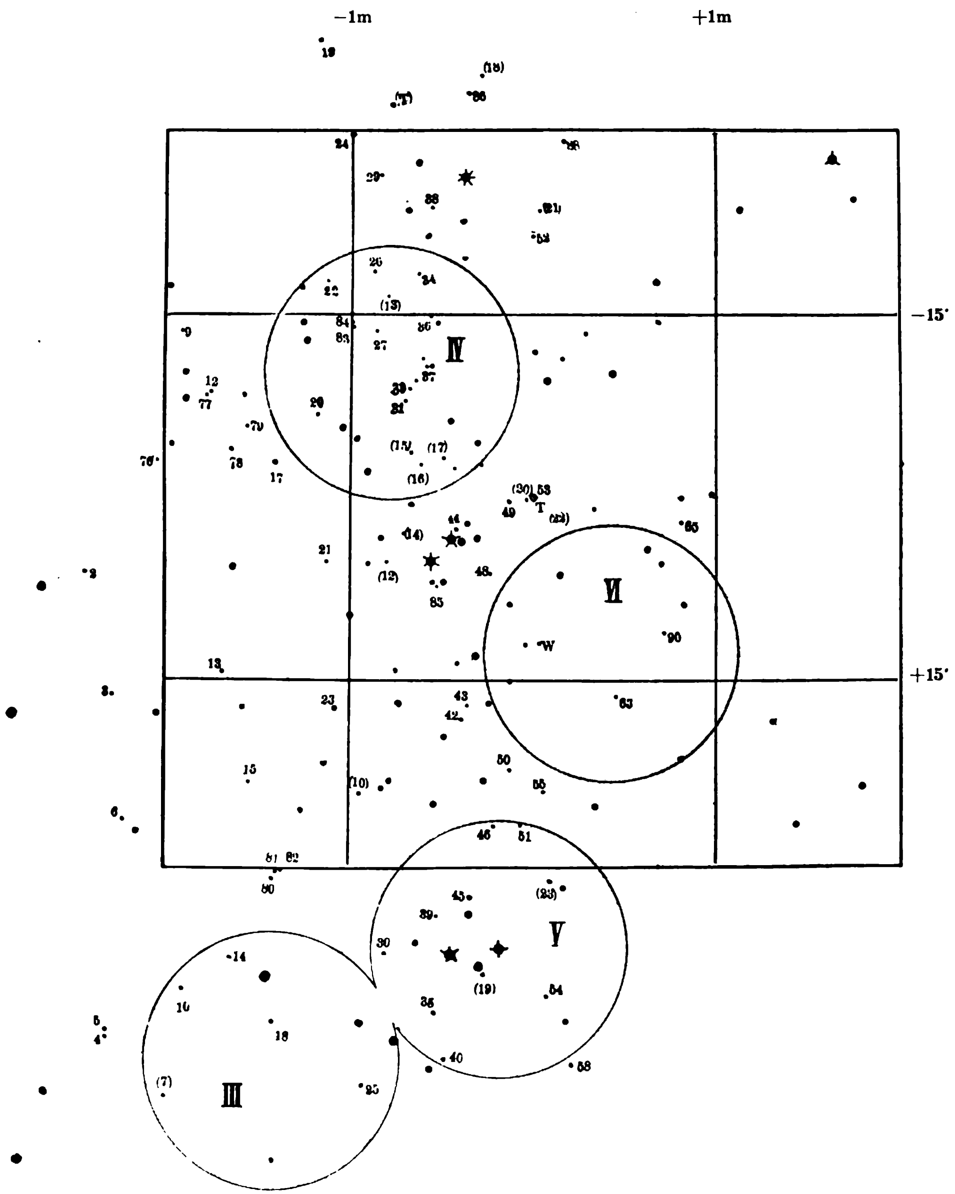
By John G. Hagen, S. J.

In Volume **19** of this Journal (pp. 344–349) a list of stars was published as a basis for a chart which might serve as a guide in observing the many variables recently discovered in the star cluster of the *Orion* nebula. The Atlas[1] Chart for *T Orionis* seemed to suit this purpose, if somewhat extended and filled in. This chart, however, is not supposed to make other more detailed charts unnecessary; it is only intended to locate the circular charts of Wolf, and to indicate the places of the variables given in these charts and in the Harvard *Circulars*. The positions of all the stars on the chart are published either in the Atlas Catalogue or in the list mentioned above, and its supplementary tables, which follow below. No attempt was made at inserting stars from available photographs by eye-estimates. The chart, therefore, will alone not suffice to identify all the new variables, but will serve as a guide in using the charts of Bond and of Wolf. The star disks of the new variables are relatively too large, but easily distinguishable by the appended Harvard numbers.

Since the publication of the Harvard *Circular* No. 78, two more *Circulars* have appeared, No. 79 and No. 86, with additional discoveries and measures of variables in the same region. In Table II of No. 86 there are eight new variables, which, with eight *B. D.* stars not yet contained in our first list, are tabulated in the supplementary tables below. We have thus on this chart $73+8=81$ variables, more or less confirmed.

The four circular charts of Wolf, which fall within this chart, are indicated by circles and marked III, IV, V, VI, according to Wolf's numbering. Instead of the numbers (6), (8), (9), (24), (25), which referred to five stars not yet confirmed as variable in *Circular* 78, the corresponding new numbers 76, 78, 80, 88, 90 of *Circular* 86 are printed on the chart. The star marked *W* is Wolf's b_3, and (*T*) denotes Bond 539, to which Pogson had applied the letter *T*.

[1] This refers to the author's *Atlas Stellarum Variabilium*, Berlin, 1899.—Eds.



EXTENSION OF HAGEN'S CHART FOR *T ORIONIS*.

The two stars Bond 642 and 654, which Mr. J. A. Parkhurst[1] has found to be variable, are too near the bright star θ^1 to be printed on the chart.

The three stars Nos. 5, 6, 8 of Table I in *Circular* No. 86, which were suspected by Isaac Roberts, are not inserted, as requiring further confirmation or identification.

SUPPLEMENTARY TABLES.

TABLE I.				TABLE II.			
P.	Chart	$\Delta\alpha$	$\Delta\delta$	*B. D.*	Mag.	$\Delta\alpha$	$\Delta\delta$
77	....	$-1^m\ 49^s$	$-\ 8'.3$	$-5^\circ.1287$	9.3	$-2^m\ 58^s$	$+18'.5$
79	....	$-1\ \ 36$	$-\ 5.6$	89	8.5	$-2\ \ 43$	$+\ 7.7$
81	....	$-1\ \ 26$	$+31.0$	91	9.5	$-2\ \ 11$	$+27.3$
82	....	$-1\ \ 25$	$+31.0$	92	9.3	$-2\ \ \ 4$	$+17.9$
83	IV	$-1\ \ \ 1$	-13.7	93	9.8	$-2\ \ \ 0$	$-\ 4.0$
84	IV	$-1\ \ \ 1$	-14.0	$-4^\circ.1164$	8.0	$-2\ \ 56$	$+54.1$
85	....	$-0\ \ 31$	$+\ 7.6$	65	9.0	$-2\ \ 39$	$+50.2$
86	....	$-0\ \ 23$	-33.0	82	9.2	$-0\ \ 35$	$+46.6$

As in the previous table, the heading "P" refers to the numbers of Professor Pickering, and the heading "Chart" to Wolf's circular charts; the magnitudes are taken from the *B. D.* and the places are relative to *T Orionis* in the center of the orginal chart.

GEORGETOWN COLLEGE OBSERVATORY,
October 1904.

[1] ASTROPHYSICAL JOURNAL, **20**, 136–139, 1904.

Minor Contributions and Notes.

A DIVISION OF THE STARS IN SOME OF THE GLOBULAR STAR CLUSTERS, ACCORDING TO MAGNITUDE.[1]

THE program of observations for the Crossley reflector undertaken by the late Professor Keeler contained the following eight well-known globular star clusters:

N. G. C.	R. A.	Decl.
5272	$13^h\ 38^m$	$+28°\ 53'$
5904	15 13	+ 2 27
6205	16 38	+36 39
6218	16 42	− 1 46
6656	18 30	−24 00
7078	21 25	+11 44
7089	21 28	− 1 16
7099	21 35	−23 38

An exposure of $1^h\ 30^m$ to 2^h was required to bring out the faint stars which, in telescopes of ordinary size, give the appearance of nebulosity to these clusters.

The general appearance of these clusters is very similar; they are of nearly the same angular dimensions, and the magnitudes of the component stars are remarkably alike.

In each of these clusters practically all of the stars can be separated into two classes of magnitudes. Perhaps a third of the whole number lie between the eleventh and thirteenth photographic magnitudes, while almost all of the remainder are very faint, being about the sixteenth magnitude. The appearance is that of two layers, one of bright stars superposed upon another of very faint stars.

In his study of the *Hercules* cluster (*M* 13), Palmer[2] notes this division of magnitudes and gives the number of "bright" stars in that cluster as 1016, and of "faint" stars as 4466.

No numerical magnitudes are assigned, owing, probably, to the difficulty of determining magnitudes with sufficient accuracy from the Crossley

[1] Also to appear in a *Bulletin* of the Lick Observatory.

[2] "The Distribution of Stars in the Cluster *Messier* 13, in *Hercules*," ASTROPHYSICAL JOURNAL, **10**, 246, 1899.

photographs. Scheiner[1] gives the magnitudes of the 833 stars in this cluster catalogued by him as from 12 to 14.

From visual observations of the cluster *Messier* 13 with the thirty-six-inch refractor, I should assign, for the brighter stars, a range of about 2 magnitudes, from 11 to 13, and of one magnitude, $15\frac{1}{2}$ to $16\frac{1}{2}$, for the faint stars. There is little departure from these ranges for the other clusters. But few stars of magnitudes 14 to $15\frac{1}{2}$ are to be found in these clusters.

Photographs of the ω *Centauri* cluster, obtained at the Harvard College Observatory station at Arequipa, exhibit the same division of the stars in that cluster into two groups as does the Crossley series. The limits of the area of the faint stars are fully as sharply defined as those of the brighter ones and the centers of the two groups coincide. We are, therefore, led to the conclusion that, in each case, the observed division is in the same group of stars in space. That it is the characteristic of all clusters of this type is not certain, but the lack of any exceptions in the clusters observed suggests such a hypothesis.

The clusters under consideration are widely distributed, covering eight hours of right ascension and over seventy degrees of declination. Only one of them (*N. G. C.* 6656) is in the Milky Way.

In the case of the *Hercules* cluster, Palmer found that the faint stars appeared to be distributed, approximately uniformly, in a sphere having a nine-minute radius, whereas the bright stars are more numerous near the center of the cluster. This result for one cluster, coupled with the general appearance of all clusters, leads to the belief that the real form of these objects (at least the nine in question) is spherical.

Two hypotheses, to account for the peculiar distribution in magnitude, suggest themselves:

1. That it is due to a difference in the size of the stars.
2. That it is due to a difference in constitution or physical condition.

The almost complete lack of physical data at present prevents any useful discussion of these hypotheses. Although the first appears to be the more probable, yet it is conceivable that this peculiar distribution might result from a difference in constitution of the stars themselves.

The suggestion has been made that an absorbing medium pervades the cluster. It is difficult to see how such an appearance as that noted could result from absorption of the light, upon any reasonable assumption as to the character and distribution of such a medium. The result of such absorption, in a cluster of globular form, should cause a general diminution

[1] "Der Grosse Sternhaufen im Hercules *Messier* 13," *Abhandlungen der k. Akademie der Wissenschaften zu Berlin*, 1892.

in the brightness of the stars as we approach the center of the cluster. No such effect is to be detected, however.

In this connection, attention may be called to the relation which has been supposed to exist between the nebulæ and the star clusters. The belief has been gaining way, since photography has shown the real structure of so many of those objects, that they are but different stages in the process of evolution; that a star cluster has been formed by the condensation of the matter in the nebula.

A study of the nebulæ which have been observed with the Crossley reflector shows that a large proportion are spiral, and that practically all the spirals are lenticular or disk-shaped. Many of them are relatively very thin. Now, if the globular clusters are really spherical, as seems probable, it is difficult to see how they could have originated from a disk-shaped nebula (spiral?).

As there are other forms also of nebulæ and star clusters, it is not necessary to assume that the order of evolution mentioned above is the only one. But the natural tendency has been to connect such changes as these with the spirals which, by their appearance, seem to indicate greater systematic internal activity than others.

C. D. PERRINE.

MOUNT HAMILTON, CALIFORNIA,
July 17, 1904.

THE NUMBER OF THE NEBULÆ.[1]

PROFESSOR KEELER, soon after beginning his program of work with the Crossley reflector, showed that the number of nebulæ is very much greater than had been supposed. He conservatively placed the number within reach of that telescope at 120,000.

His program comprised the taking of photographs of 104 of the brighter nebulæ and clusters located in all parts of the sky within reach of the telescope, *i. e.*, north of declination $-25°$. The recent completion of this program enables us to revise his estimate.

In 57 of the regions 745 *new nebulæ* have been discovered. Almost all of them are very small and faint. The regions in which no new ones were found were, as a rule, those surrounding the clusters and very large nebulæ. There were 142 known nebulæ observed, in these regions, making the total number observed 887, an average of $8\frac{1}{2}$ per region. As it would take 62,000 such photographs to cover the entire sky, the results indicate 500,000 as the corresponding number of nebulæ within reach of the Crossley

[1] Also to appear in a *Bulletin* of the Lick Observatory.

reflector. This assumes that the small portion observed represents fairly the entire sky. It is well known that the nebulæ are much more numerous in some parts of the sky than in others. This is a tendency which, so far as we know, affects large and small nebulæ alike.

The fact that a considerable number of other subjects than the nebulæ (presumably non-nebulous regions) are included in the program, indicates that the portion observed is fairly representative of the whole sky.

Longer exposures, more sensitive plates, and more perfect photographs will undoubtedly reveal some nebulæ which do not now appear, and others which are confused with the faint stars. It seems probable, therefore, that the number of the nebulæ will ultimately be found to exceed a million.

The positions of the new nebulæ discovered on the Crossley photographs have been determined, and a catalogue of them will be printed in the volume of reproductions of nebulæ and star clusters, soon to be issued.

C. D. PERRINE.

MOUNT HAMILTON, CALIFORNIA,
June 18, 1904.

THE NINTH SATELLITE OF *SATURN*.[1]

IT is probable that in the future there will be no difficulty in securing a sufficient number of observations of *Phoebe*, the Ninth Satellite of *Saturn*, not only to correct the present elements, but to study the large and interesting perturbations to which it is subject. It can be observed visually with the largest refractors, and can doubtless be photographed with large reflectors, as well as with the Bruce telescope, by the aid of which Professor William H. Pickering discovered it. Since the observations enumerated by him in the Harvard *Annals*, **53**, 55, 60, *Phoebe* has been closely followed by Professor Bailey. The approximate positions obtained by him with the Bruce telescope are given in Table I, and the positions found by Professor Barnard visually on August 8 and September 12, 1904, with the

TABLE I.

Positions of *Phoebe*.

Desig.	Date	G. M. T.	Exp.	x	y	Desig.	Date	G. M. T.	Exp.	x	y
	1904	h m	m	′	′		1904	h m	m	′	′
A 6771	June 18	18 20	120	+24.20	+6.65	A 6844	Aug. 4	14 44	..	+10.86	+1.61
A 6773	20	18 30	120	+23.84	+6.65	A 6846	5	14 32	..	+10.60	+1.37
A 6801	July 6	16 15	120	....	......	....	8	18 0	..	+ 9.70	+0.08
A 6804	7	16 14	120	....	......	A 6854	15	10 15	..	+ 6.83	+0.15
A 6807	11	16 58	180	....	......	A 6856	16	18 51	..	+ 6.40	0.00
A 6841	Aug. 2	14 47	...	+11.56	+1.85	......	Sept. 12	12 30	..	+ 5.93	−4.28

[1] *Harvard College Observatory Circular* No. 87.

forty-inch Yerkes telescope, and announced in the Harvard *Bulletins* Nos. 157 and 159, are added, to bring together all the material so far collected. The designation of the plate, the date, Greenwich Mean Time, exposure, and rectangular co-ordinates referred to *Saturn* as an origin, are given in the successive columns.

For some unexplained reason, *Phoebe* has not been found on the plates taken in July. The record for the plates taken in August, has not yet been received from Arequipa.

A NEW VARIABLE IN *HERCULES*.

The meridian photometer, like other meridian instruments, is not adapted to the discovery of variable stars. It may therefore be of interest to note the discovery of such an object by the writer, with the twelve-inch Meridian Photometer. On August 23, 1904, while measuring the star $+24^\circ.3419$, mag. 9.4, it was noticed that a brighter star, having the photometric magnitude 9.5, and not in the Bonn Durchmusterung, preceded it. An examination, the next day, of the photographs of this region at once showed that the star was a variable of long period having a range extending at least from the magnitude 9.5 to <13. The approximate position for 1855, is R.A., $18^h\ 20^m\ 26^s.0$; Dec., $+24^\circ\ 56'.4$.

EDWARD C. PICKERING.

SEPTEMBER 12, 1904.

REVIEWS

The Moon: A Summary of the Existing Knowledge of Our Satellite, With a Complete Photographic Atlas. By WILLIAM H. PICKERING, of Harvard College Observatory. Pp. viii + 103, with 100 illustrations. New York: Doubleday, Page & Co., 1903. Price $10.

IT is a fact that the Moon has been badly neglected visually, in recent years, and that its study has been relegated to the amateur with small instrumental means. The large telescopes of today have never seriously taken up its study. Yet there is perhaps no object in the sky that would more probably repay the careful observer than a close study of the Moon's surface, but this evidently must be a study of details, because only in such is it likely that any discoveries of importance would be made. We have perhaps come too early to the conclusion that the Moon is a dead world and that no real changes take place upon its surface. But in the light of the powerful telescopes of today this might be a very rash conclusion. That its study has not been intelligently taken up with modern telescopes is perhaps due to the fact that, for one thing, it appears too easy and too commonplace. Another reason possibly may be that its great brightness would be injurious to the eyes of an observer who wished also to observe other and especially faint objects. This, coupled with the tradition that it was dead and that no changes took place upon its surface, perhaps produced the foregone conclusion that it would be a waste of time to take up the study of the Moon seriously.

Whatever the explanation of this neglect may be, it is true that observers in general had evidently come to the conclusion that the Moon was an object fit only to show to the necessary visitor, or to be execrated for spoiling the night with its undesired brilliancy as the Sun had already partly done by cutting out a large portion with which to make the day.

The Moon, therefore, it would seem, might offer a rich field for careful and original investigation with sufficiently powerful telescopic means.

Professor William H. Pickering has taken up this subject in recent years, and his results are so startling —bordering, as they do, on the sensational— that one hesitates to accept them, or rather to accept his conclusions.

He claims to have found evidences, not only of present volcanic life, but of snow and ice, clouds and vegetation.

Perhaps if these discoveries had come slowly and one at a time, with long intervals between, they might have been received with better grace; but they have been turned out by wholesale, and almost any place on the Moon would seem to be conspicuously productive of one or more of the above phenomena; even the fateful canals of *Mars* are found to be denizens of the Moon also.

Professor Pickering's results have been published in the large quarto volume which has been placed in my hands for review.

The book is conveniently separated into various chapters dealing with the origin of the Moon, in which the researches of George Darwin are popularly retold: the reasons for believing the Moon's atmosphere must be very attenuated—its small mass making it impossible for the Moon to prevent the escape from it of the gases necessary to make an atmosphere; the origin of the lunar craters, in which he shows that many of the theories that have been offered to account for them are untenable—such as the impact of huge meteorites when the Moon was in a plastic condition, etc. He finally comes to the conclusion that there is a strong resemblance in many of the volcanic features of the Hawaiian islands to lunar phenomena, and concludes that the lunar craters have been formed by processes not materially different from those on the Earth, except where vaster results have come from the smaller mass of the Moon.

One chapter treats of vegetation and the lunar canals. The author concludes that, though a certain scant vegetation exists on the Moon, no intelligent life can exist there, and indirectly proves that the canals on *Mars* cannot be due to the work of man.

> Sixthly, since no water in the liquid form can exist on the Moon, this fact will enable us to rule out many seductive but erroneous hypotheses. Seventh, and lastly, we know that so little air and water vapor exist there that we can confidently also rule out all aid in the construction of these formations [the "canals"] from intelligent or intellectual life . . . . they merely weaken the strongest argument hitherto found for the existence of highly intelligent life upon *Mars*.

In this chapter Professor Pickering gives comparisons, side by side, of drawings and photographs of certain craters to prove the existence of the lunar canals. The photographs used in this comparison are so excessively enlarged that the details are mere blotches and it is hard to say what they represent. In these pictures, plates E and F, the resemblance between the drawings and the photographs are certainly very vague, and an interested mind could possibly trace out the canals or any other desired feature, but to the unimpassioned mind the various canals shown in the drawings have to be imagined on the photographs.

A chapter devoted to the formation of artificial craters gives many interesting experiments in which lunar-like formations are produced. Perhaps by such experiments some idea may be obtained of the possible formation of the lunar craters. There is danger, however, of coming too readily to conclusions that may be wrong, from the very fact that any experiments must be on such a minute scale, compared with the forces that have been at work on the Moon, that a comparison might be entirely misleading.

The author concludes, however, that the *maria* were never the abode of bodies of water, and that they are not, therefore, the beds of former oceans, as some have supposed; and also that their formation was perhaps due to the solidification of great lakes of molten matter after the surrounding regions had solidified.

In reference to the ice and snow, which Professor Pickering finds on the Moon, there seem to be large regions covered with this so-called ice and snow. In Plate C he gives two reproductions from the same full Moon negative in which one is printed out until only the very brightest regions remain. By this method Professor Pickering endeavors to separate the "snow"-covered regions from the ordinary bright, so-called volcanic regions. By this heavy printing there are left regions of extra brightness, while the generally bright regions have nearly all disappeared, leaving the snow areas alone visible. Is not this a trick of photography itself in which the very brightest regions are differentiated from the rest by excessive overprinting? Might not this as readily be carried farther and still brighter regions alone be left, and, if so, what would they be called? How these residual bright regions thus revealed can be called "snow" does not yet seem quite clear. It would appear to be simply a survival of the brightest.

Speaking of the bright rays from Tycho, Professor Pickering says:

> What has hitherto been considered one of the strangest features is that they are never visible at lunar sunrise or sunset, but require that the Sun shall have an altitude of at least 5° or 10° in order to render them visible. This peculiarity we have already explained as being due simply to the fact that the snow which forms them lies in crevices instead of on a smooth surface. The Sun must therefore necessarily attain a certain altitude before they can become visible.

But at such altitudes of the Sun these supposed crevices should themselves become visible from the shadows within them. As they are not so visible, it will be necessary to find some other means of hiding the snow near the time of sunrise.

Professor Pickering instances the crater Linné as perhaps the best evidence of changes still taking place on the lunar surface. The history

of this object is well known. The earlier astronomers designate it as a small crater of considerable size and depth. Since Schmidt's observations of 1866, when it seemed to have disappeared to him, it has been a small and rather difficult crater, and must have been beyond the reach of some of the earliest telescopes that described it. It is not now one-sixth the size formerly attributed to it. After the Sun has risen upon Linné for a few days, the crater-like formation gives place to a rather large nebulous spot of light, several times the size of the real crater. This luminous spot, Professor Pickering finds, becomes smaller as the Sun rises higher upon it. He attributes this appearance to a deposit of hoar frost about the crater during the lunar night (in some way due to the activity of the crater) and the subsequent melting of it by the Sun. If this is frost so formed, it is not quite clear why it should not be visible when the Sun first shines upon it. Is it not as probable that some peculiarity of the immediate surface surrounding the crater, along with the aid of the Sun's light at certain angles, is responsible for the phenomena? A more conclusive proof of Professor Pickering's idea would be from observations of this object immediately before and after an eclipse of the Moon. Such observations he has made. Measures of the diameter of this spot of light were obtained by him at the eclipse of December 16, 1899. His measures showed that the spot increased $0\overset{''}{.}14$ in diameter through its immersion for some two and a half hours in the shadow of the Earth. This spot is, however, so excessively ill-defined in a telescope that the measures of its diameter may well differ by $1''$ of arc. Therefore it would be an even chance that a difference several times his value would be bigger or smaller in the measures of its diameter on such an occasion. But at the eclipse of October 16, 1902, Professor Pickering found an increase in size apparently due to additional accumulations about the crater while in the shadow of $2\overset{''}{.}8$, which is a very measurable quantity in even a vague spot like that around Linné. There is no question but that this spot does apparently decrease in size with the increase of the Sun's altitude and afterward increase as the Sun goes down, as Professor Pickering has claimed for it.

Professor Pickering finds numerous other cases of change which he believes to be due to present volcanic activity. These observations are so startling that until they are fully verified one hesitates to accept them as real. Some of these phenomena consist of shifting white streaks or areas which he attributes to "streams of gas issuing from the craters and carrying with them white crystals of snow, thus forming real clouds upon the Moon."

The author's accounts of the origin of the Moon, its motions, phases, etc.; why one face of the Moon is always turned toward us; the probability

of an atmosphere, and the reasons he gives for believing such an atmosphere to be very thin, etc., are contained in chapters 1, 2, and 3. They are interesting reading and are clearly written, and must be of value to the ordinary reader.

Chapter 10 is also interesting as giving an account of the various superstitions, etc., connected with the Moon.

Some of the other chapters contain matter that would be interesting to the general reader, but where the author has gone into minute details of his own observations and conclusions, with frequent reference to the photographs, the ordinary reader would find it difficult to follow him.

The book is really the result of an expedition undertaken by Professor Pickering in 1900 and 1901 to the island of Jamaica, in latitude +18°, with the special object of studying and charting the Moon with a telescope 12 inches in diameter and of 135 feet 4 inches focus, which gave a 16-inch image of the Moon.

This instrument was mounted on the slope of a hill so that the axis of the telescope pointed to the north pole of the heavens, the lens remaining stationary. The light from the Moon was thrown into the object-glass by a movable flat mirror 18 inches in diameter, which was made to revolve westward at the rate of one revolution in twenty-four hours.

After experimenting with the instrument, it was found best to reduce the aperture to 6 inches, and with this essentially all the photographs were made. This extremely great ratio of aperture to focus—1:270—was necessarily very slow, besides interfering seriously with the separating power of the instrument. Even with the bright Moon the exposures in some cases were of two minutes' duration.

With this telescope eighty photographs were obtained (during the seven months' work) which covered the Moon's surface completely five times. These photographs, which are bound in the last part of the volume, are reproduced of the original size, only a portion of the Moon's surface, however, being shown on each plate. There are five plates of each region, and these are under different illuminations.

A study of these same regions under different illuminations is highly interesting and instructive. They show most strikingly the effect of varying illumination on craters and streak systems. Especially interesting are the full Moon phases.

The definition is seldom good in these photographs. Doubtless this is due to unsteadiness during the long exposures made necessary by the relatively small aperture. Though this seriously detracts from a close study of details, yet it does not materially affect their study as a chart and for the examination of the general features.

Following these are the real charts—by quadrants. These are on a large and convenient scale. On them are drawn lines of parallels and meridians. For each of these four pictures is a corresponding hand-drawn chart on which are printed the names of the different objects, from which they are easily recognized on the photographs. Though these four photographs are lacking in definition and have had, for the sake of exactness, the principal objects outlined by hand, they are, nevertheless, of great value for the location and study of details on better photographs. An accompanying table gives the approximate longitudes and latitudes of the various objects, with their names.

For convenience in comparison with the text, it would have been better to have bound the photographs and charts separately from the text.

The following list of errata has been printed by the publishers:

ERRATA

Page 32, line 31, for 5.7 read 5.6.

Page 37, line 18, for (64,159) read (2.5, 6.3).

Page 37, line 25, Plate 3B is inverted. These measures should therefore be made from the left and top.

Page 37, line 29, for 6.3 read 6 2.

Page 37, line 31, for 6.6 read 6.5.

Page 39, line 5, for 4.1 read 4.2.

Page 43, line 16, for 8 read δ.

Page 49, line 19, for B read C.

Page 49, line 25, for Messier, A read Messier A.

Page 52, line 2, see page 37, line 25, above

Plate F. The straight dark lines on Figs. 6 and 7 are defects. Fig. 7 should be turned one-quarter way around, so as to resemble Fig. 8.

Page 68, line 18, for 33 read 13.

Page 70, line 27, for 3B (1.2, 8.5) and 3E (1.1, 8.6) read 2A (3.2, 1.2) and 2C (3 2, 1.8).

Page 70, line 28, for 5 and 8 read 3 and 6.

Plate H, last line, for 22 read 2.

Page 103, line 1, for contents read constants.

Plate 3B. This plate is inverted.

E. E. B.

Observations of Variable Stars Made at the Rousdon Observatory, Lyme Regis, under the Direction of the late Sir C. E. PEEK. Edited by H. H. TURNER. *Memoirs of the Royal Astronomical Society*, Vol. **55**, 1904.

THIS valuable memoir contains about five thousand observations of twenty-two long-period variable stars, mostly northern circumpolar, made

between 1887 and 1901. They have been carefully edited and thoroughly discussed by Professor Turner, and the resulting volume is one of the most complete that have ever appeared in this branch of astronomy, and may well serve as a model for future publications of variable star work.

The series of observations is remarkable in several respects, notably for its homogeneity, its continuity, and its inclusion of (nearly) complete light-curves of the variables. The stars observed were:

T Cassiopeiae	*S Ursae Majoris*	*T Draconis*
S Cassiopeiae	*S Boötis*	*R Cygni*
S Persei	*R Camelopardalis*	*χ Cygni*
R Aurigae	*S Coronae*	*S Cygni*
U Orionis	*R Ursae Minoris*	*T Cephei*
R Lyncis	*R Draconis*	*S Cephei*
R Ursae Majoris	*S Herculis*	*R Cassiopeiae*
T Ursae Majoris		

As far as the aperture of the telescope (a Merz refractor of 6.4-inch objective) permitted, the variables were followed throughout their cycles, which gives great value to the series. The observations were all made by C. Grover, assistant at the private observatory. The methods of observation were notable in two respects: The variable was usually compared with five neighboring stars, therefore large magnitude intervals were often used, frequently more than a magnitude, and sometimes two, three, or even four magnitudes. Evidently the method cannot be called "Argelander's," and it is a question whether the large intervals were not better omitted in cases where at least two comparison stars nearly equal to the variable were included. The hour of observation was not recorded, so that difficulty was met in tracing the effect of varying hour angle on the estimates.

In regard to the appearance of the stars, the following quotation from the author's introduction is interesting:

Great attention has been devoted to the visual physical appearance presented by these variables. It was soon found that they differ in a remarkable degree from ordinary stars. Most of them are of a deep red or ruddy color, and many are more or less nebulous. They may be divided into four classes, viz., stars having—

a) A remarkably well-defined, almost planetary disk

b) Well-defined stars surrounded by a more or less dense, ruddy atmosphere.

c) Large woolly stars, with ill-defined image, resembling a small but bright planetary nebula.

d) Stars which at minimum show, in place of the variable, a slight bluish nebulosity.

The reviewer has observed fifteen of these stars with telescopes ranging from six to forty inches in aperture, but has been unable to confirm this nebulous appearance, either with the twelve-inch Brashear or the forty-inch Clark refractors, or by visual or photographic observations with the six-inch or twenty-four-inch reflectors. How much of this appearance may have been due to the Merz objective is a question.

The reductions and discussions by Professor Turner are worthy of the highest praise. The limits of this review permit the mention of only a few salient points:

1. The magnitudes of the comparison stars were based on the photometric values given in *Annals of the Harvard College Observatory*, Vol. **37**, but so combined with the results of the Rousdon comparisons as to represent that work better, while keeping the general Harvard scale. This permits a direct comparison with the simultaneous Harvard observations of the same stars; perhaps the most important fruit so far borne by Professor Pickering's efforts toward co-operation in this field.

2. To give a "bird's-eye" view of the results, the mean light-curve for each variable is formed, and the deviations of the separate curves are given in tabular form—a very compact and satisfactory method, showing at a glance the star's behavior and permitting a deduction of the correction to the assumed period.

3. Light on the quality of the observations is therefore thrown from two sources—the deviations from the mean curve and the comparison with the simultaneous Harvard observations. Curious results follow: first, the deviations from the mean curve are disappointingly large, frequently more than a full magnitude both in the Rousdon and Harvard work; second, the large deviations in the one series are not usually confirmed by the other; thus the discordances between the series are still larger, amounting at times to more than a magnitude for slightly colored stars and more than two magnitudes for the deep red stars. One is at a loss to account for such large discordances (similar though shorter series known to the reviewer differ by three tenths of a magnitude at most), but they lead the editor to the weighty conclusion:

4. "We are thus led to the important conclusion that the apparent deviations from a mean curve are due to the observer and not to the star; or, in other words, the variations of brightness in the variable are perfectly regular, though the difficulties of observing introduce apparent deviations." The editor is thus led to undertake a preliminary harmonic analysis of the light-curves, but, though the results are promising, he concludes: "We are in face of a problem of some complexity, which will require the careful

discussion of all the material available, and not merely of one set of observations; and such a discussion cannot be entered upon here."

5. Two things strongly urged by the editor for future work are: first, the publication of detailed comparisons of the variables; second, the publication of mean light-curves. A careful study of the foregoing work fully justifies these recommendations.

P.

NOTICE.

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention will be given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed.

Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

If a request is sent *with the manuscript* one hundred reprint copies of each paper, bound in covers, will be furnished free of charge to the author. Additional copies may be obtained at cost price. No reprints can be sent unless a request for them is received before the JOURNAL goes to press.

The editors do not hold themselves responsible for opinions expressed by contributors.

The ASTROPHYSICAL JOURNAL is published monthly except in February and August. The annual subscription price for the United States, Canada, and Mexico is $4.00; for other countries in the Postal Union it is 18 shillings, 6 pence. Correspondence relating to subscriptions and advertisements should be addressed to *The University of Chicago, University Press Division, Chicago, Ill.*

Wm. Wesley & Sons, 28 Essex St., Strand, London, are sole European agents, and to them all European subscriptions should be addressed.

All papers for publication and correspondence relating to contributions and exchanges should be addressed to *Editors of the* ASTROPHYSICAL JOURNAL, *Yerkes Observatory, Williams Bay, Wisconsin, U. S. A.*

INDEX TO VOLUME XX.

SUBJECTS.

INDEX TO VOLUME XX.

AUTHORS.